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The martyr Nicholas Ridley, bishop of London from 1551 to 1553, who was burned at the stake on October 16, 1555. From a painting by an unknown artist, reproduced courtesy of the National Portrait Gallery, London.

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## Evolution of Medieval Mentalities: A Cognitive-Structural Approach

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CHARLES M. RADDING

IN BOOK II of the *History of the Franks*, Gregory of Tours recounted how Clovis annexed the kingdom belonging to his kinsman, Sigibert the Lamé. Clovis began by promising his support to Sigibert's son Chlodéric, if Chlodéric would kill his father. After assassins murdered Sigibert while he slept, Chlodéric informed Clovis of the deed and invited him to dispatch men to receive a portion of Sigibert's treasures. Clovis answered with assurances of his good wishes, but the men Clovis sent to Chlodéric killed him as he bent over to display the coins in a treasure chest. Clovis then came to the district himself to address the populace. According to Gregory, Clovis in his speech disclaimed any knowledge of the affair. "I cannot," he explained, "shed the blood of my own kinsman, which it is shameful to do." Clovis offered to place the lands of the slain kings under his protection. The populace answered Clovis's speech with great applause of "both voices and shields," raised him up on a shield, and acclaimed him their king.<sup>1</sup>

The story itself is a bizarre enough tale of perfidy, but Gregory's conclusion is perhaps even stranger since he, Christian though he was, expressed no shock at or disapproval of the treacheries and lies he had just described. Instead, Gregory asserted that Clovis had succeeded because "God was laying low his enemies every day under his hand and was increasing his kingdom, because Clovis walked with an upright heart before God, and did what was pleasing in His eyes." To Gregory Clovis's very success may have been proof of his virtue, or Gregory may have believed Clovis was guiltless, since he shed no blood with his own hands but acted through agents. However Gregory's judgment is interpreted, it is evident that to him Clovis's actual motivation or intention had little bearing on the morality of his action. Nor was this the only episode for which Gregory showed little concern for the intentions of the individuals whose actions he narrated. When Gregory described the murder of Chilperic, for example, he recounted in considerable detail where the assassination occurred, at what time of day it happened, Chilperic's posture as he was stabbed, where the knife entered his body, how the blood spurted forth—everything, in fact, except who the assassin was and

<sup>1</sup> Gregory, *Liber Historia Francorum*, bk. 2, chap. 40.

why he wished to kill the king.<sup>2</sup> Similarly, in the episode of the feud between Sicharius and Chramnesindus, Gregory treated in detail the murder of the servant whose death triggered the quarrels although, as Erich Auerbach has noted, to provide a motive for the incident “would seem to have been more important than to tell us that the servant fell before he died!”<sup>3</sup> Indeed, much of the sensuousness of Gregory’s style, on which Auerbach commented so insightfully, may derive from the virtual exclusion of speculation about motivation from his narrative in favor of purely material details.

GREGORY OF TOURS’S ATTENTION TO PHYSICAL BEHAVIOR at the expense of intention was typical of much of early medieval society. The Benedictine Rule, for example, defined the steps of humility primarily in terms of behavior. St. Benedict wrote of ascending and descending the ladder of humility “by our acts” and urged monks to avoid voicing complaints, to laugh but rarely, and to walk with their eyes cast down toward the ground.<sup>4</sup> To be sure, the details Benedict gave are better selected than some of those Gregory offered, but they still lack the careful attention paid to motivation in late classical treatises like Augustine’s *Confessions*. And the practice of monasticism reveals a much more fundamental neglect of intention and interiority. This neglect is strikingly apparent in the widespread custom of offering young children as oblates to monasteries to be raised as monks, a method of recruitment that filled monasteries with men who had never themselves chosen to be monks. To demand high standards of asceticism or spirituality was hardly possible in these circumstances, and the love of God was commonly conceived of as “the due and regular performance of external obligations.”<sup>5</sup> Monks and patrons alike believed that piety demanded only the observance of a precise daily routine, and as late as the eleventh century they considered the performance of liturgical ritual to be the essence of monastic dedication.

The confusion—perhaps one should say the interchangeability—of intention and behavior is also evident in secular law. In the *Leges Henrici Primi* of 1114–18, drawn primarily from Anglo-Saxon codes, the compiler discussed homicide—in scattered fashion throughout many different sections—without differentiating among intentional slayings, deaths caused by negligence, and those resulting from unavoidable fault. Generally, the penalty for homicide

<sup>2</sup> *Ibid.*, bk. 6, chap. 46.

<sup>3</sup> Auerbach, *Mimesis*, trans. Willard Trask (Princeton, 1953), 83. For his general discussion of Gregory’s style, see *ibid.*, 77–95; and, for Gregory’s narration of the incident, see his *Liber Historia Francorum*, bk. 7, chap. 47.

<sup>4</sup> *Regula S. Benedicti*, cap. 7. On this and what follows, see R. W. Southern, *The Making of the Middle Ages* (New Haven, 1953), 223–25, 156–63, and *Western Society and the Church in the Middle Ages* (Baltimore, 1970), 217–28. Benedict probably derived his ladder of humility from the somewhat earlier *Regula Magistri*, but the precise origin of the passage does not affect the present argument. For the text of the *Regula Magistri*, see the edition by A. de Vogüé, 3 vols. (Paris, 1964–65); and, for a discussion of the relationship between the two rules, see David Knowles, *Great Historical Enterprises* (London, 1963), 135–66.

<sup>5</sup> David Knowles, *The Monastic Order in England* (2d ed., Cambridge, 1963), 684. Knowles has commented that in general the monks were devoted to a “superficial material idea of the *servitium dei*”; for oblation, see *ibid.*, 418–20.



was owed "in circumstances in which a man cannot lawfully swear that a person was not through his agency further from life or nearer to death."<sup>6</sup> The compiler included cases that seem strange to us: "if anyone sends for a person and the latter is killed while coming; if anyone, when summoned to a place by a person, suffers death there; if anyone, being brought to witness a public exhibition of a wild beast or a madman, incurs some injury at their hands; if anyone entrusts a horse or other thing to a person and thence some harm befalls him." The explanation of these rules, according to the compiler, was the maxim *qui inscieniter peccat, scieniter emendat*: who unknowingly commits a wrong knowingly shall make amends.<sup>7</sup> The compiler himself seems somewhat uncomfortable with the rule (one legal scholar has suggested that in this respect the compiler was in advance of his time<sup>8</sup>) and tried to offer a few cases in which mercy rather than the rigor of the law should be applied, but these he could describe only in terms of physical events: Henry falls out of a tree, landing on Charles, who dies; Henry should be quit of feud or compensation, but, if Charles's kin insist on exacting revenge, they can do so by falling out of a tree on Henry.<sup>9</sup> A modern observer can see that these materialist circumstances permit an inference of innocence of intention, but the compiler of the *Leges Henrici Primi* does not himself state such an abstract principle.

Few questioned these legal or monastic practices for the six centuries before 1050. After that time, however, these rules and assumptions were increasingly challenged and discarded: the law described in the *Leges Henrici Primi* virtually disappeared in the half century following its compilation; the Cistercians abandoned oblation around 1100 and it was moribund well before the Fourth Lateran Council finally prohibited it in 1215.<sup>10</sup> In each case change was directed toward greater concern with the interior aspects of human nature. Because monasticism and law could only function with the support of medieval elites, these shifts also indicate changes in the collective mentality of European society.

These trends in mental attitudes have been examined by many historians—among them Friedrich William Maitland, Dom David Knowles, Marie-Dominique Chenu, R. W. Southern, and, most recently, Colin Morris<sup>11</sup>—but none

<sup>6</sup> L. J. Downer, ed. and trans., *Leges Henrici Primi* (Oxford, 1972), cap. 90, 11b.

<sup>7</sup> *Ibid.*, cap. 90, 11c; cap. 88, 6a; cap. 90, 11a.

<sup>8</sup> E. Kaufmann, *Die Erfolgshaftung*, Frankfurter Wissenschaftliche Beiträge, no. 16 (Frankfurt, 1958), 100.

<sup>9</sup> *Leges Henrici Primi*, cap. 90, 7, 7a.

<sup>10</sup> The Cistercians' *Consuetudines* (ca. 1099–1109) forbade accepting novices under the age of fifteen. The last oblation at Abington occurred around 1150, and in 1168 the community of St. Augustine's, Canterbury, asked for and received a papal injunction against the acceptance of children by their superior. Knowles, *Monastic Order in England*, 212, 421. Also see Pierre Riché, "L'enfant dans la société monastique au XII<sup>e</sup> siècle," in *Pierre Abélard/Pierre le Vénérable: Les courants philosophiques, littéraires, et artistiques en occident au milieu du XII<sup>e</sup> siècle* (Paris, 1975), 689–701, esp. 693.

<sup>11</sup> F. Pollock and Maitland, *The History of English Common Law*, 2 vols. (2d ed., Cambridge, 1898); Knowles, *Monastic Order in England*; Southern, *Making of the Middle Ages*; Chenu, *La théologie au douzième siècle* (Paris, 1957), partially trans. by Jerome Taylor and Lester K. Little as *Nature, Man, and Society in the Twelfth Century* (Chicago, 1968), and *L'éveil de la conscience dans la civilisation médiévale* (Montreal, 1968); and Morris, *The Discovery of the Individual 1050–1200* (New York, 1972). The list could be extended considerably; for example, Benjamin Nelson has stated that the twelfth and thirteenth centuries "constituted the prime seedbeds of the institutional and culture developments of the Western world. Indeed, . . . when these centuries are considered in international perspective, they prove to have been a watershed in the international history of the world." See his

has succeeded in giving an adequate account and many have retreated to vague generalities. Sometimes the matter has simply been sidestepped by indefinite references to "social and economic change." Another approach has related the new ideas in law and religion to an intellectual renaissance of the eleventh and twelfth centuries, on the theory that the innovations were connected with the wider use of reason in human affairs. But more intellectual activity does not necessarily mean different thoughts. Equally plausible is the argument that dissatisfaction with old attitudes stimulated the growth of scholarly debate. The changes have also been attributed to the new institutions of the twelfth century. Knowles has suggested that oblation disappeared "when the monks had ceased to be the only teachers and educated class," for "it was inevitable both that the monasteries should pay less attention to their children and that boys educated outside the cloister should wish to become monks." Perhaps sensing the frailty of this explanation, which begs more questions than it answers, Knowles continued by pointing to the criticisms directed against the old practices by the Cistercians; their standards, he argued, had to be met by rival monastic houses.<sup>12</sup> His explanation supposes that anyone would recognize the superiority of the Cistercian ideal, but, in fact, early medieval monastic patrons consciously opposed it. Their notion of strict discipline excluded any monastery whose monks were ill-clothed and whose liturgy left ample time for solitude.

Some legal historians have challenged the idea of a major shift in attitudes toward intention by denying that the Germanic codes accurately reflected the values of early medieval society. In their view, the reliance on physical circumstances for assessing degrees of culpability resulted from the difficulty of weighing the internal workings of the mind. "Ancient law could not discuss the question of intent because it had not the machinery wherewith to accomplish the inquiry."<sup>13</sup> Admittedly, the early Middle Ages lacked sophisticated means of judicial proof, but ordeals and other "judgments of God" were eminently suitable for determining questions of intention. A believer in ordeals could conceivably argue that in such cases a recourse to divine judgment was preferable to the use of human reason, since God could know intention and men could not. There is, moreover, little sign that people then had serious doubts about the oaths and ordeals they used. Ethnographers have found that contemporary societies that use ordeals place great confidence in the results, and medieval courts sometimes rejected rational proofs that were available in favor of ordeals.<sup>14</sup> King Alfred's ordinance on slayings by spear carriers also

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"*Eros, Logos, Nomos, Polis: Their Changing Balances and the Vicissitudes of Communities and Civilisations*," in Allan W. Eister, ed., *Changing Perspectives in the Scientific Study of Religion* (New York, 1974), 85-111, esp. 56 (Nelson's emphasis).

<sup>12</sup> Knowles, *Monastic Order in England*, 421.

<sup>13</sup> M. Bateson, *Borough Customs*, vol. 2, Selden Society, no. 21 (London, 1906): xl.

<sup>14</sup> Several Carolingian capitularies strictly limited the use of witnesses, and Louis the Pious provided for recourse to judicial duel if contradictory testimony were produced. F. L. Ganshof, "La preuve dans le droit franc," *La Preuve*, Recueils de la Société Jean Bodin, no. 17 (1965), 85-87. For contemporary primitive cultures, see L. Levy-Bruhl, *La mentalité primitive* (5th ed., Paris, 1960), 244-50.

belies the supposed inadequacy of procedure: an offender who impaled his victim with a spear held in a safe manner over the shoulder was less culpable than one who carried the spear pointed ahead and presumably could see anyone approaching. In the former case the bearer owed *wer* to the victim's family but could purge himself of the payment of *wite* to the king by an oath that the homicide was not intended.<sup>15</sup> Alfred's law remains a solitary instance of a judicial inquiry into intention rather than merely physical events, but, it demonstrates that, had intent been thought crucial to justice, oaths or ordeals would have been thought adequate proofs.

The failure of historians to confront the reality and significance of early medieval materialism appears to stem from the belief that such attitudes are contradictory to basic human nature. Oliver Wendell Holmes summed up the matter in commenting that "even a dog distinguishes between being stumbled over and being kicked." This truism was sufficient evidence for Holmes to assert that, contrary to the explicit statements of the laws themselves, unintentional wrongs were not prosecuted in Anglo-Saxon England. Percy Winfield took the same position in a famous article on liability in English law. "No human being, ancient or modern," he wrote, "needs any mental education beyond that of general experience to say 'A did not mean to do this,' and therefore to inflict a lighter penalty or possibly none at all."<sup>16</sup>

THE WORK OF THE GREAT SWISS COGNITIVE PSYCHOLOGIST, Jean Piaget, has, however, cast doubt upon the certainty of historians regarding this issue. Piaget pointed out that the lack of interest in intention exhibited in primitive law resembles the attitudes toward rules—called "moral realism"—that are typical of children in all societies.<sup>17</sup> According to Piaget, moral realism has at least three features: (1) the belief that any act that shows obedience to a rule is good and that any act that does not conform is bad; (2) the rule is not taken as something to be judged and interpreted but as something that is given, "ready made and external to the mind," so that the letter and not the spirit of the rule is obeyed; and (3) acts are evaluated in terms of their conformity with the rule

<sup>15</sup> Alf., cap. 36. According to A. W. G. Kean, evidence on child liability also "supports the theory of strict liability. If a child injured me, I took vengeance upon him simply because he had done me wrong; his tender years may have lessened my fury, but nevertheless I would take vengeance upon him." Kean, "The History of Criminal Liability of Children," *Law Quarterly Review*, 53 (1937): 365. Since no elaborate procedures would have been necessary to exempt minors from penalties, holding children liable reinforces my contention that the practice of strict liability reflected a judgment that he who harmed should pay.

<sup>16</sup> Holmes, *The Common Law* (Boston, 1881), 1–38; and Winfield, "The Myth of Absolute Liability," *Law Quarterly Review*, 42 (1926): 37. Winfield mustered a number of examples to show that Anglo-Saxon law did not strictly adhere to the doctrine "a man acts at his own peril" but concluded that the laws were so confused and unsystematic as to defy any generalization. For different interpretations of the evidence, see Kaufmann, *Erfolgshaftung*, and F. B. Sayre, "Mens Rea," *Harvard Law Review*, 45 (1932): 975. Sayre quoted a Visigothic law that "whoever shall have killed a man, whether he committed the homicide intending to or not intending to (*volens aut nolens*), . . . let him be handed over into the *potestas* of the parents or next of kin of the deceased"; after a review of the evidence, Sayre concluded that "up to the twelfth century the conception of *mens rea* in anything like its modern sense was non-existent." Sayre, "Mens Rea," 977, 981.

<sup>17</sup> Jean Piaget and Bärbel Inhelder, *The Psychology of the Child*, trans. H. Weaver (New York, 1969), 126.

and not according to the motive that prompted them. Piaget's studies of how children conceived of lies reveals the practical effects of these attitudes. Piaget presented his subjects with paired stories. In one story a child who had not been examined at school told his mother that he had received good marks; she rewarded him. In the other story a child who was frightened by a dog told his mother he was chased by "a dog as big as a cow." Young children (those under eight) typically said that the two children were equally bad or that the second child was worse. That the first child intended to deceive and the second did not was not taken into account. Instead, the crucial issue was the response of the adult; some of Piaget's subjects believed the first child had done nothing wrong because his mother believed him. Most young children also thought it was all right to tell lies to other children, since they were not capable of inflicting punishment and would believe the lie. In contrast, children over ten usually thought the first child, who had lied about the marks, was worse because he had lied to deceive his mother into rewarding him. They dismissed the lie about the dog not only because there was no intent to deceive but because no one would believe it. Older children also thought it was as bad to lie to children as to adults or worse, because children are little and likely to believe a lie.<sup>18</sup>

Piaget's inquiries concerning attitudes toward clumsiness and stealing also reveal the tendency of small children to focus on objective circumstances, such as the size of the lie or the giving of rewards. In one story a child climbed into a cupboard to get some jam and knocked over a cup, which fell and broke. In the other story a child was called to dinner. He opened the door to the dining room, but behind the door on a chair was a tray of cups he could not have known about. The tray fell and fifteen cups were broken. Piaget found that most young children (ages six to seven) paid attention to the amount of damage—in this case breaking fifteen cups was worse than breaking one—whereas older children (those over ten) were interested primarily in intention. Children between seven and ten often used both criteria, judging objectively at one time and subjectively—according to intention—at another.<sup>19</sup>

The reliance of young children on objective moral criteria carries over into their attitudes toward rules. Unable to grasp the intention of the rule maker, they interpret rules literally and do not vary them to meet circumstances. Piaget found this trait even in games played by children away from adult influence. He discovered that, when children first become aware of rules, they treat them as "sacred and untouchable." Exceptions favoring younger children by, for example, permitting them to shoot closer when playing marbles, are considered to be impossible; as one small child said, "God would make the little boy's shot not reach the marbles and the big boy's shot would reach them." About the age of ten a belief that rules are made by agreement of the players replaces this belief that divine justice opposes any change in the rules.

<sup>18</sup> Jean Piaget, *The Moral Judgment of the Child*, trans. M. Gabain (New York, 1948), 139–74.

<sup>19</sup> *Ibid.*, 155–56.

"In short," as Piaget put it, "law now emanates from the sovereign people and no longer from the tradition laid down by the Elders." An awareness of the spirit of the game or fair play that is not formulated in terms of rules also appears at roughly the same age, and exceptions for the sake of equity become possible. This less rigid attitude toward rules is founded on an increased ability to understand the other person and to cooperate on the basis of mutual expectations instead of literal rules, and greater flexibility thus reflects the development of subjective responsibility: "when the child is accustomed to act from the point of view of those around him, when he tries to please rather than to obey," then "he will judge in terms of intentions. So that taking intentions into account presupposes cooperation and mutual respect."<sup>20</sup>

Despite the obvious similarities between the reasoning of tenth-century adults and that of modern children, many historians may question whether the parallels are more than coincidental. There are, after all, inevitable differences between adults who act in the greater world and children whose experience is largely confined to family and neighborhood. The medieval adults may be responding to the pressure of external circumstances when their thought resembles that of children. To evaluate the kinds of reasoning used by the authors of historical texts, furthermore, presents a methodological problem. We have no way of posing dilemmas to Gregory of Tours, St. Benedict, or St. Bernard. We must take them as we find them.

The work of Lawrence Kohlberg, who with his associates refined and extended Piaget's theory, has a bearing on these objections. Kohlberg devised hypothetical dilemmas that could be used to test the reasoning of adults as well as children. A typical dilemma concerns a man who steals a drug for his sick wife because he cannot afford to buy it. To assess the morality of the man's action, the subject must consider issues such as the value of human life, property rights, the importance of interpersonal relationships, and respect for the law. Using these dilemmas Kohlberg has demonstrated the existence of six sequential stages of moral reasoning. The first two (preconventional) stages correspond to Piaget's moral realism: stage one shows an orientation toward punishment and stage two, toward exchange and reprisal. The next two (conventional) stages show a respect for the opinions of others, first by the value the subject places on interpersonal relationships (stage three) and then by the value he places on society as a whole (stage four). The final stages represent abandonment of conventional attitudes entirely; emphasis on human rights and social contacts (stage five) and universal principles of justice (stage six) replace earlier values and patterns of thought.<sup>21</sup>

<sup>20</sup> *Ibid.*, 58, 72, 137.

<sup>21</sup> For the works appropriate to the discussion here, see Kohlberg, "Stage and Sequence: The Cognitive-Developmental Approach to Socialization," in David A. Goslin, ed., *Handbook of Socialization Theory and Research*, 347-480, esp. 376-89, "The Development of Children's Orientations toward a Moral Order, I: Sequence in the Development of Moral Thought," *Vita Humana*, 6 (1963): 11-33, "From Is to Ought: How to Commit the Naturalistic Fallacy and Get Away with It in the Study of Moral Development," in T. Mischel, ed., *Cognitive Development and Epistemology* (New York, 1971), 151-235, and, with K. Kauffman, P. Scharf, and J. Hickey, *The Just Community Approach to Corrections: A Manual*, pt. 2, chap. 5: "Understanding and Diagnosing Moral Stages" (Moral Education Research Foundation, Harvard University).



Research has confirmed Kohlberg's contentions that individuals pass through the moral stages in an order that does not vary from society to society and that the differences between the stages are developmental instead of cultural. Psychologists are not yet certain what determines the moral stage attained by an individual at adulthood, but social experience rather than written culture or religious doctrine apparently provides the dominant influence. A person does not learn to maintain the order of society by instruction from his parents or other superiors—such interaction could as easily result in preconventional obedience to rules without regard for their social value—but by experiencing situations that make him value society and feel a part of it. Societies differ widely in the kind of opportunities they offer for social interaction—which may explain why there are considerable differences between societies in the kinds of moral reasoning that are most common. Kohlberg found that, whereas approximately one-fifth of American sixteen-year-old boys continued to use reasoning of the kind Piaget discovered in children (Kohlberg's stages one and two), more than half of the sixteen-year-old boys in modern peasant societies used those kinds of reasoning.<sup>22</sup> It also follows that changes in social organization such as those that began in eleventh-century Europe could conceivably generate changes in the way a society's members interpreted their cultural heritage.

The cognitive theory of moral development runs counter to certain assumptions about human nature that historians often uncritically make. First, if children achieve adult moral reasoning only after passing through a lengthy developmental sequence, socialization cannot simply be the internalization of specific values or their inculcation by conditioning. Piaget made this point by stressing that by its nature learning is active: a child does not copy adult thought, he *reconstructs* it. The transmission of moral values from generation to generation is not, therefore, comparable to the imitative learning of dance steps or of the words to a song. A certain variation that cannot be ascribed either to faulty socialization or to deviance is to be expected.

Cognitive theories also suggest new ways of understanding cultural change. Individuals can, for example, adopt new beliefs without changing their moral stage. The conversion of Germanic Europe to Christianity may have been an instance of this process, since the substitution of Christian rituals for pagan ones was achieved without the Christian doctrine of forgiveness of enemies having much effect on the practice of feuding. It is significant in this connection that Kohlberg has found that the Christian rule "Do unto others as you would have them do unto you" cannot be understood until morality is evaluated in terms of interpersonal relationships (stage three); otherwise, the rule is interpreted as condoning revenge—"Do unto others as they do unto you." Piaget also stressed that the rejection of revenge comes not from

<sup>22</sup> Kohlberg, "Stage and Sequence," 385, 397–404. Also see Elliot Turiel, "Developmental Processes in the Child's Moral Thinking," in Paul H. Mussen *et al.*, eds., *Trends and Issues in Developmental Psychology* (New York, 1969), 122–25; and Carolyn P. Edwards, "Societal Concepts and Moral Development: A Kenyan Study," *Ethos*, 3 (1975): 505–28.

external constraint but as part of a change in cognitive orientation that rejects revenge as inadequate, because “there is no end” to it.<sup>23</sup>

Cultural change can also take place by a reinterpretation of traditional values and texts. Humility was a Benedictine and Christian virtue throughout the Middle Ages, but until the twelfth century it was conceived of as faithful obedience to the rule. Odo of Cluny, for example, was known as “the Digger” because, in accordance with Benedict’s twelfth step of humility, Odo always kept his eyes toward the ground.<sup>24</sup> By contrast, when St. Bernard discussed the twelfth step in his treatise on Benedict’s ladder, he denounced the lifted head that was a sign of curiosity that turned a monk away from introspection to investigation of the world, but he approved lifting up one’s eyes to seek help from others or to offer it. Bernard himself doubtless believed he was merely recapturing Benedict’s original sense, but his work effectively presented a new way of understanding old virtues; as Southern has put it, the work “abounds in new definitions of familiar words, new arrangements of old thoughts, and new insight into states of mind,” all the while retaining the familiar forms.<sup>25</sup> What changed between Odo and Bernard was not the list of virtues or even the venerated text but the reasoning by which those virtues and that text were brought to life.

ANALYZING MENTALITIES IS OBVIOUSLY DIFFERENT from analyzing the thought of individuals, and an investigation must begin with the study of common opinion. For the Middle Ages this opinion almost invariably means that of the social elite, but, if this limitation is accepted, three kinds of evidence assume particular importance. Legal and penitential codes are valuable indicators of community standards in any society, and both underwent suggestive changes in the period under consideration. Monastic practices are significant for the same reason, because monks were recruited from, and supported by, lay society. Finally, the attitudes of literate classes can be directly approached through the writings of those such as Bernard, whose influence in their own times suggests they addressed the concerns of their contemporaries. Taken together, these sources permit us to perceive in rough outline the cognitive structures that underlay medieval mentalities. Although it may eventually be possible to discuss in these terms the causes of the twelfth-century change in mentalities, the present purpose is primarily to offer a preliminary description.

For the early Middle Ages the Germanic codes are of special importance because they contain many folk or popular elements and therefore are likely to

<sup>23</sup> Kohlberg, “From Is to Ought,” 197–98, and *Moral Judgment of the Child*, 323.

<sup>24</sup> Barbara Rosenwein, “Rules and the Rule at Tenth-Century Cluny,” *Studia Monastica*, 19 (1977): n. 23. Rosenwein argues that Odo and Cluny in general were devoted to a rigorous observance of external forms such as wearing habits of a certain color or maintaining the vow of silence even when interrogated by Viking marauders.

<sup>25</sup> Bernard, “De Gradibus Humilitatis et Superbiae,” X, 28–29, in Jean LeClerq and H. M. Rochais, eds., *S. Bernardi Opera*, 3 (Rome, 1963): 38–39; and Southern, *Making of the Middle Ages*, 230.

represent the views of ordinary people. One feature of interest is the heavy emphasis upon compensation for injuries—*bot* and *wer* in Anglo-Saxon law. The codes provided elaborate tariffs for homicides and injuries short of homicide. One Anglo-Saxon code allowed thirty shillings for an ear, sixty shillings for an eye, the tongue, a hand, or a foot, eight shillings for a front tooth, and so forth.<sup>26</sup> The indemnities served as a substitute for revenge by the victim or his kin, a feud to which the rest of society was a neutral bystander or, at best, a mediator.<sup>27</sup> This conception of justice as exchange between autonomous individuals provides a striking parallel to Kohlberg's stage two subjects, one of whom suggested that a judge could properly punish a man who stole an expensive drug by fining him an amount equal to the price of the drug and giving the money to the druggist.<sup>28</sup> The more expensive wergilds assigned to higher-ranking persons, furthermore, may indicate not merely the hierarchical nature of society (many consciously hierarchical societies, including those of later medieval Europe, make no legal distinctions between free persons<sup>29</sup>), but also the greater importance early medieval men placed on the variety in the conditions of men instead of on their shared humanity. Again, there is an echo in Kohlberg, who found that stage one subjects typically acknowledge a differential value of life and believe it is worse to kill an important or wealthy person than someone with less stature.

Also of interest are the harsh penalties exacted in Germanic law for secret crimes. Although ordinary homicide was subject to no special penalties even when premeditated, *morth* (Latin: *murdrum*) or secret homicide was punished by forfeiture of life. And, unlike modern law, which considers robbery an aggravated form of theft because it entails danger to persons as well as property, Germanic law considered theft worse than robbery and subject to a higher rate of compensation because taking a man's property behind his back was worse than assaulting him in an open fight where he had a chance to defend himself.<sup>30</sup> Underlying these provisions was a moral consciousness that could not with certainty value life over property but that had a keen interest in fairness.

Distinct from this scheme of compensations to peers were the indemnities owed authorities. In Anglo-Saxon law these were called *wites*. Some actions had to be paid for by *bot* or *wer* to the injured party and *wite* to the king and

<sup>26</sup> Alf., cap. 44–77.

<sup>27</sup> For an example of an attempt to mediate, see Gregory's role in the feud between Sicharius and Chramnesindus where the Church paid the composition that was due. An interesting feature of this episode is that it was enough for Chramnesindus to be paid even though not by Sicharius, who thus went scot-free. Efforts at this kind of mediation appear to have been rare, however, and this one ended in disaster with a renewal of the feud. Gregory, *Liber Historia Francorum*, 7, 47, 9, 19. Royal mediation of feuds was also rare; see J. M. Wallace-Hadrill, *The Long Haired Kings and Other Studies in Frankish History* (London, 1962), 129.

<sup>28</sup> Kohlberg, "Understanding and Diagnosing Moral Stages," 36.

<sup>29</sup> Pollock and Maitland, *English Common Law*, 1: 408–09.

<sup>30</sup> Maitland commented, "In the days of the blood feud . . . mere deliberation cannot have been thought an aggravation of the crime; a man was entitled to kill his enemy provided that he was prepared to pay the price or bear the feud, but he was expected to kill his enemy in a fair, open honest manner, not to take a mean advantage, not to fall upon him like a thief in the dark." *The Collected Papers of Frederic William Maitland*, ed. H. A. L. Fisher (Cambridge, 1911), 328. For theft, see P. D. King, *Law and Society in the Visigothic Kingdom* (Cambridge, 1972), 251–58; Pollock and Maitland, *English Common Law*, 2: 493–94.

possibly to the owner of certain rights of jurisdiction. *Wite* could be exacted for violence against churches, infringements of the king's peace (*grith* or *mund* in Anglo-Saxon law), or, most generally, disobedience to the king's law. Depending on the offense, the *wite* could take the form of either a fixed sum of money or payment of wergild; in the latter case, the *wite* was evidently thought of as an alternative to forfeiture of life, for the wergild varied according to the status of the offender and not—as in cases of *bot*—the status of the injured party. Moreover, some offenses could not be emended by money payment and the penalty was death unless the king granted a special pardon.<sup>31</sup>

It is easy to see in these provisions for *wite* a primitive conception of the state as a community, but two important aspects of early medieval perceptions of authority contradict that impression. First, the provisions for *wite* generally depended on the king as a person and not on the crown as an institution. The king's peace in Anglo-Saxon law, for example, was only applicable to special individuals or special places—some documents stress this by referring to “handgiven peace”—and it lapsed on the death of the king. Only later was the king's peace thought of as covering the whole of the realm and extending through periods of interregnum.<sup>32</sup> Correspondingly, one of the main penalties of the age was to become the *inimicus* of the king. This primitive form of outlawry had its parallel in provisions of Frankish law that placed a malefactor *extra sermonem regis*, and in England was usually accompanied by a decree of excommunication. Although later English law made outlawry a routine procedure and hedged it in by restrictions, Anglo-Saxon outlawry seems to have required a special act of the king and could be cancelled only by a royal act of mercy.<sup>33</sup>

The scheme of *wites* also reveals that—just as Piaget's children thought of the rules of their own games as coming from outside agencies such as older children, parents, or God—in early medieval law legitimacy was thought of as coming from outside society. The codes portray a sacral kingship in which the image of the king tended to merge with that of God. In many cases of violence against the Church, *wites* were owed the king, and in general the king was treated as occupying a position similar to that of a bishop. The king was said to be Christ's deputy (*Cristes gospelsia*)—the same Anglo-Saxon term used for a Benedictine abbot—and the phrase “Christ and the king” was frequently employed in the dooms. Alfred, for example, stated that everyone ought to love his lord as Christ loved God.<sup>34</sup> A somewhat similar usage appears in Merovingian and Carolingian Gaul, where the king often referred to “*Dei et nostra gratia*” (God's and our grace) or “*Dei iram . . . et nostram offensam*”

<sup>31</sup> Pollock and Maitland, *English Common Law*, 2: 458, 483; and *Leges Henrici Primi*, cap. 12.

<sup>32</sup> Pollock and Maitland, *English Common Law*, 2: 463–64.

<sup>33</sup> On Anglo-Saxon outlawry, see F. Liebermann, “Die Friedlosigkeit bei den Angelsachsen,” *Festschrift für Brunner*, 17–37, with the corrective provided by J. Goebel, *Felony and Misdemeanor* (New York, 1937), 44–61, 419–23.

<sup>34</sup> W. A. Chaney, *The Cult of Kingship in Anglo-Saxon England* (Berkeley and Los Angeles, 1970), 156–247, esp. 185–86, 197–98, 194–95.

(God's anger and our enmity) or otherwise verbally linked themselves to God. Such usage was not merely semantics, for the kings often regranted their *gratia* to subjects who were vindicated by an ordeal or "judgment of God." Royal *gratia*, moreover, was conceived to be as wholly arbitrary as was God's; agents and subjects received the powers, goods, or privileges granted to them not by any right of theirs but because the king freely granted them, and thus there was no awareness that public life could consist of reciprocal duties.<sup>35</sup> Law, therefore, was conceived of as coming from above and not, as Bracton later put it, from "the common engagement of the *res publica*."<sup>36</sup>

Historians often explain the character of the Germanic codes by pointing to early medieval society: in the absence of strong government the blood-feud served as a deterrent to violence and compositions helped to minimize the feuds. But many features of the law codes recurred in books prepared to guide priests in assigning penances, where the availability of coercive force was not a consideration. The penitentials specified a fixed period of penance for each tersely defined sin: little scope was left for the discretion of the priest or for consideration of special circumstances, and, in contrast to the handbooks of twelfth- and thirteenth-century confessors, there was no interest in probing the mind of the sinner and obtaining interior repentance.<sup>37</sup> Instead of emphasizing *contritio*, the emotional aspect of penance, the penitentials were almost invariably concerned with *satisfactio*, the actions by which reparation was offered God for the sin.<sup>38</sup> God was thought of as rigorous in his demands for payment, and one had to be particularly careful to pay one's debts to him, even to the point of atoning for sins about which an offender may not have had certain knowledge.<sup>39</sup> Fortunately, the materialist conception of penance meant that it mattered little whether the sinner performed these acts in person or arranged for a third party to do them; much of the importance of early medieval monasteries lay in their role as substitutes for the performance of the penance for laymen.<sup>40</sup>

<sup>35</sup> F. L. Ganshof, "La 'Gratia' des monarques francs," *Anuario de Estudios Medievales*, 3 (1966): 22.

<sup>36</sup> "In frühen Mittelalter kann ein Gesetz zwar ohne die Zustimmung des Volkes Rechtskraft gewinnen, niemals aber ohne die Zustimmung des Königs"; Ekkehard Kaufmann, *Aequitatis Iudicium: Königsgericht und Billigkeit in der Rechtsordnung des frühen Mittelalters*, Frankfurter Wissenschaftliche Beiträge, no. 18 (Frankfurt, 1959): 131. Kaufmann has also commented, "Frühes Recht lebt in der Einheit von lex und iustitia, eine Einheit, die durch den sakralen Charakter dieses Rechts bedingt ist." For Bracton, see S. Thorne, ed., *De Legibus et Consuetudinibus Angliae*, 2 (Cambridge, 1968): 71.

<sup>37</sup> "De la fin de l'âge patristique au XII<sup>e</sup> siècle un Penitentiel est un tarif, un barème, qui donne une liste de tous les péchés possibles, péchés graves s'entend dans la discipline sacramental d'alors, en indiquant en regard l'importance, nature et durée, de la penance publique qui doit être imposée au coupable ... surtout ils frappent par leur automatisme, le prêtre qui les utilise ne joue aucune rôle personnel dans l'application de leur prescription, il n'a pas même la latitude d'appréciation que les codes modernes laissent au juge dans la fixation des peines." Pierre Michaud-Quantin, *Sommes de casuistique et manuels de confession au moyen âge (xii-xvi siècles)*, *Analecta Mediaevalia Namurcensia*, no. 13 (Louvain, 1962), 15. For examples, see T. McNeill and H. M. Gamer, *Medieval Handbooks of Penance* (New York, 1938).

<sup>38</sup> Chenu, *L'éveil de la conscience*, 23.

<sup>39</sup> For example, the penitential for those who fought at the battle of Hastings specified what to do if one had wounded an opponent and was uncertain whether he had subsequently died; David C. Douglas, ed., *English Historical Documents*, volume 2: 1042-1189 (London, 1953), 606-07. I am indebted to Charles Wood for this reference.

<sup>40</sup> Southern, *Western Society and the Church*, 225-27. For the role of third parties in feuds, see note 27, above.



Compared with secular law, the penitentials do appear to show more concern for intention, at least in occasionally raising the issue of intent mitigating the severity of penance. These provisions, however, are deceptive. Almost invariably they merely repeated canons or decrees of the ancient church, as did nearly all of the articles of the penitentials. If one looks elsewhere in the early medieval literature on penance for original statements (that is, neither copied nor paraphrased) on the nature of penance or the importance of contrition, one is again defeated; Halitgar and Rabanus Maurus, for example, derived their long tracts almost entirely from earlier authorities, and the force of their arguments depended upon the reader's willingness to accept patristic texts without question. The penitentials, therefore, provided not an original moral sense, but an affirmation of belief in authority.

This same attitude toward authority typifies early medieval intellectual life in general. Doctrine was to be accepted literally and uncritically as if, in Piaget's terms, it were something exterior to the mind instead of having a logical structure to be reconstructed by one's own intelligence. Isidore of Seville, the leading scholar of the seventh century, apparently thought that faith was compatible only with blind acceptance of dogma. He derived the word heresy from the Greek for "choosing, because, namely, each one chooses for himself what seems better to him. . . . But it is permitted to us to establish nothing out of our own will, nor to choose what someone has established out of his will. We have the apostles of God as authorities, who did not choose out of their own will anything that they believed but passed on to the nations the doctrine received from Christ."<sup>41</sup> Isidore's contemporaries were of the same opinion: Gregory the Great took the Bible as a higher authority on Latin usage than the treatises of ancient grammarians,<sup>42</sup> and Gregory of Tours indicted Chilperic for, among other faults, daring to offer his own opinion about the nature of the trinity.<sup>43</sup>

The Carolingian Renaissance had little impact on these intellectual methods. Although a few authors—notably Paschase Radbertus and John Scotus Erigena—suggested new interpretations or ventured to question the views of the Fathers, most scholars devoted their efforts to editing and preserving old texts. In their own works the Carolingians generally were content to collect quotations related to their subjects. To study most Biblical commentators of the period, Beryl Smalley has written, "is simply to study their sources," and sometimes the anthologies of quotations even failed to name the Father whom

<sup>41</sup> "Haeresis Graece ab electione vocatur, quod scilicet unusquisque id sibi eligat quod melius illi esse videtur. . . . Nobis vero nihil ex nostro arbitrio inducere licet, sed nec eligere quod aliqui de arbitrio suo induxerit. Apostolos Dei habemus auctores qui nec ipsi quicquam ex suo arbitrio, quod inducerent, elegerunt, sed acceptam a Christo disciplinam fideliter nationibus adsignaverunt." Isidore, *Etymologiarum sive Originum*, ed. W. M. Lindsay (Oxford, 1911), bk. 8, sec. 3, 2–3.

<sup>42</sup> *Patrologia Latina* (hereafter *PL*), 75, 516b, as cited by Jacques Fontaine, *Isidore de Seville et la culture classique dans l'Espagne wisigothique*, 1 (Paris, 1959): 34; Since Gregory considered the Holy Spirit entirely responsible for the book of Job, he thought its human author an irrelevancy as if "we were reading the words of some great man . . . yet were to inquire by what pen they were written." Quoted by Beryl Smalley, *The Study of the Bible in the Middle Ages* (2d ed., Oxford, 1952), 33.

<sup>43</sup> Gregory, *Liber Historia Francorum*, bk. 6, chap. 46.

the author excerpted.<sup>44</sup> This method was not confined to sacred studies: Jonas of Orleans justified his reliance on authorities in his book on royal power by claiming that “our words are of little value” since in patristic texts one can “behold perpetually as if in a looking glass what one ought to be, to do, and to avoid.”<sup>45</sup> Given this reluctance to look beyond the words of ancient texts, it is not surprising that when disputes arose argument consisted “principally of the two sides bombarding each other with a mass of quotations from Scriptures and the Fathers of the Church.”<sup>46</sup>

The evidence for early medieval mentalities therefore suggests that concepts of justice or right were dependent on the notion of transcendent authority. King, priest, and the Bible received their legitimacy not from human institutions but from God, and obedience to them all was enforced by the threat of divine punishment. Obedience, moreover, meant conformity to literal precepts, physically in the case of law and doctrinally in intellectual endeavors. As Piaget has shown, judgment according to objective or material criteria instead of intention follows naturally from this literal approach to rules and authority.

This materialist mentality was predominant in Europe for over five centuries, nor is there any reason to believe that it would have broken down from internal contradictions. In the eleventh century, however, new ideas appeared in several different fields. First, heresies that questioned the nature of baptism developed. The heretics denied the efficacy of ritual alone, and argued that intellectual understanding and commitment were preconditions of an effective baptism.<sup>47</sup> Second, Archbishop Wulfstan in England made a clear statement on judgment by intention in a homily included in the laws of Aethelred. Legal institutions did not themselves change, of course, but the passage deserves mention as possible evidence of more advanced moral reasoning.<sup>48</sup> By the middle of the century there were monastic communities like Grandmont, whose members were noted for their intensely personal devotion and who treated their rules not as a sacred text but as the customs of their house.<sup>49</sup> Finally, the eleventh century witnessed explorations into logic. This innovation should not be overstated, for as late as Lanfranc logic was used with extreme caution;<sup>50</sup> but the trend is significant as an effort to determine the underlying meaning of sources instead of treating them as definitive pronouncements.

These intellectual changes were probably related to the quickening pace of economic growth and social change in the eleventh century, but the nature of

<sup>44</sup> Smalley, *Bible in the Middle Ages*, 37–38.

<sup>45</sup> Quoted by J. Reviron, *Les idées politico-religieuses d'un évêque du ix<sup>e</sup> siècle: Jonas d'Orléans et son "De institutione regia"* (Paris, 1930), 113.

<sup>46</sup> P. Wolff, *The Cultural Awakening*, trans. A. Carter (New York, 1969), 99.

<sup>47</sup> Jeffrey Burton Russell, *Dissent and Reform in the Early Middle Ages* (Berkeley and Los Angeles, 1965), 23–24.

<sup>48</sup> Cited by T. F. T. Plucknett, *Edward I and Criminal Law* (Cambridge, 1963), 61.

<sup>49</sup> Lester K. Little, *Religious Poverty and Profit Economy in Medieval Europe, 1000–1300* (London, forthcoming).

<sup>50</sup> R. W. Southern, *St. Anselm and His Biographer* (Cambridge, 1963), 20–26.

the connection is hard to establish. The rise of new social classes does not in itself serve as a satisfactory explanation, since aristocrats as well as town-dwellers were prominent in the cultural vanguard. Yet economic change may have been important in another way by bringing about social relationships that demanded more cooperation between members of society. The development of larger communities meant all classes had to deal more often with strangers, including merchants, papal legates, and bureaucrats. With increased commerce, dealings based on contract—a concept virtually absent from the ancient law—must have become more common and provided experience of relationships based on trust.<sup>51</sup> The peace movement may have offered opportunities for another kind of cooperation.<sup>52</sup> Piaget has suggested that division of labor within society can be a stimulus to moral thought because it renders useless standards based on a single external ideal of conduct; the problem is resolved by the growth of an inner morality that looks to the spirit of the rules governing human interaction. Piaget was led to this view by Emile Durkheim's description of the effects of division of labor on the social order, and his insight warrants further investigation.<sup>53</sup>

WHATEVER THE CAUSE, BY 1100 A MAJOR SHIFT IN CULTURAL VALUES was clearly underway. This movement was exceedingly complex and impossible to summarize in a few pages, but a few topics merit attention here: the development of logic, the rediscovery of an ethic based on intent, the emphasis on emotion (particularly love), and a broadening awareness of the moral force of human society. On each of these issues the contrast between twelfth-century attitudes and those of the early Middle Ages parallels quite closely the shift cognitive psychologists have described for the moral reasoning of individuals.

The role of logic in the development of scholasticism is well known, but more was involved than the mastery of formal syllogisms. In particular, many logical traits that Piaget described as "formal operations" came into much more common use. These include, first, an ability to devise categories of subjects for presentation and analysis. In the Anglo-Saxon law codes laws are arranged in a helter-skelter fashion, held together by only the barest association of ideas; but in the treatise known as *Glanville* distinctions are carefully drawn and possible problems squarely faced. The difference expresses the gap between thought bound to specific cases and thought capable of abstract generalizations.<sup>54</sup> Second, there is the ability to test hypotheses by consid-

<sup>51</sup> Max Weber suggested that Western cities fostered a quality of fraternization different even from cities in other cultures; *The City*, trans. Don Martingale and Gertrude Neuwirth (New York, 1958).

<sup>52</sup> L. Mackinney has claimed that in the last decade of the tenth century the French clergy succeeded "in devising a peace program based upon the idea of willing cooperation in the interest of public welfare rather than upon fear of spiritual punishment"; "The People and Public Opinion in the Eleventh-Century Peace Movement," *Speculum*, 5 (1930): 184.

<sup>53</sup> Piaget, *Moral Judgment of the Child*, 342; and Durkheim, *The Division of Labor in Society*, trans. G. Simpson (New York, 1933). It should be noted, however, that Piaget disagreed sharply with Durkheim's conception of conscience as "nothing else than the collective conscience of the group of which we are a part"; *Moral Judgment of the Child*, 396.

<sup>54</sup> Plucknett, *Early English Legal Literature* (Cambridge, 1958), 3-7.

eration of imaginary situations, and, relatedly, an effort to consider all possibilities exhaustively. Thus, Abelard in his *Ethics* examined what he conceived to be the possible definitions of sin and tried them out by difficult cases. What if a monk is bound in chains next to a woman and brought to pleasure, he asks; is this sin?<sup>55</sup> Finally, logic was used to look behind the literal words of authoritative texts to the intentions of the author. Abelard made this point explicitly in the introduction to his *Sic et Non*: one should not read the words alone but should ask which of many possible meanings were intended. Through this advice Abelard sought to resolve apparently contradictory opinions. But rethinking the Fathers to regain their original meaning could easily lead to a recognition that they were occasionally fallible men on whose opinions improvements might be made. Interpretation led to innovation, a pattern that was repeated in biblical studies and law.<sup>56</sup>

Corresponding to the effort by scholars to look behind the words of their texts was the emergence of an ethic that looked behind action to intention. Abelard's chained monk was part of an argument that interior consent (*consensus* or *intentio*), not the act (*opus*), determined sin. The effect of good intention, he said, could not be bad. A theory of contritionism that flourished in the early twelfth century held that sincere repentance was all that was needed for absolution. This belief was later discarded in favor of sacramental confession, which paid more respect to the needs of the ecclesiastical community; but later scholastic philosophers continued Abelard's investigations into psychology and this became, according to Chenu, the axis of their theology.<sup>57</sup>

Bernard of Clairvaux strenuously opposed Abelard on some issues but agreed with him about the primacy of emotional and intellectual life. In his writings Bernard portrayed piety not as observance of rules but as intense personal devotion; obedience for him had importance as a sign of humility, and it did not end with conformity to the law.<sup>58</sup> (It was, after all, Bernard's order, the Cistercians, that took the lead in abolishing oblation.) For Bernard and other monastic writers of the period, moreover, love took on a special significance—William of St. Thierry called the monastery a "school of love." As J. C. Moore has recently shown, this concern with love was based on a new perception that no longer saw love as the performance of ritual to escape divine wrath. Instead, love had to be freely given and, ideally, reciprocated. Love of this sort was seen as good in itself or the source of goodness.<sup>59</sup>

The most remarkable manifestation of the importance of love in the twelfth

<sup>55</sup> *Peter Abelard's Ethics*, ed. and trans. D. E. Luscombe (Oxford, 1971), 21.

<sup>56</sup> *PL*, 178, 1339–49. For theology and biblical studies, see Chenu, *Nature, Man, and Society*, 310–30, and Smalley, *Study of the Bible*; for canon law, see S. Chodorow, *Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum* (Berkeley and Los Angeles, 1972), 140; and in general, Smalley, "Ecclesiastical Attitudes to Novelty, c. 1100–c. 1250," *Church, Society, and Politics*, 12 (1975): 113–21.

<sup>57</sup> Chenu, *L'aveil de la conscience*, 17–20, 66; and Peter Brown, "Society and the Supernatural: A Medieval Change," *Daedalus*, 104 (1975): 145.

<sup>58</sup> Chodorow, *Christian Political Theory and Church Politics*, 114–16.

<sup>59</sup> J. C. Moore, "Love in Twelfth-Century France: A Failure in Synthesis," *Traditio*, 24 (1968): 429–43. Moore has pointed out that authors of chivalric literature similarly defined love as voluntary and reciprocal and treated it as a source of goodness.

century was its effect on the way God was perceived. In the early Middle Ages, God was thought to be self-sufficient and fearsome; the motif of the wrath of God appeared everywhere. In the Benedictine Rule, for example, the abbot was cautioned seventeen times to fear God; monks, too, were exhorted to fear God "that He may not as an angry Father, disinherit His children, nor as a dread Lord . . . deliver us to everlasting punishment" and were urged to walk with their eyes constantly down to imagine themselves "arraigned before the dread judgment of God."<sup>60</sup> Beginning in the twelfth century, however, the orientation toward punishment and obedience waned as love came to be considered God's chief characteristic. Bernard explained the incarnation of Christ in terms of mutual love between God and man: "This was the principal cause why the invisible God wished to be seen in the flesh and to converse with man, that he might draw all the affections of carnal man, who were unable to love except after the flesh, to the saving love of His flesh, and so step by step lead them to spiritual love." Abelard and St. Anselm offered somewhat different accounts, but each stressed that the incarnation was an act of love, and Abelard went so far as to suggest that men should love God as a friend.<sup>61</sup> The popularity enjoyed by Bernard and Abelard testifies to the wide currency their ideas had in clerical circles; judging from the direction of legal change, similar concerns were also felt in lay society.

Innovations in law are most fully documented for England, beginning with the reforms of Henry II. Here the interest in intention was apparent by 1200, well before ordeal and battle had lost primacy among the methods of legal proof. Homicide with malice aforethought was singled out for special punishment, and pleas of self-defense and misadventure were accepted as deserving of a royal pardon; also newly entitled to pardons were children and madmen, for whom mental incompetence had not previously exempted from the penalties of compensation and feuds. The twelfth century also saw the creation of the class of crimes called felonies, which were distinguished by the charge of deliberate misdeed. The word "felony" was not, in itself, new; in earlier times it had denoted treason against one's lord. It is perhaps not too much to suggest that, with the changing moral consciousness of the twelfth century, the horror previously associated with crimes against a superior became attached to misdeeds perpetrated with evil intent.<sup>62</sup>

The separation of felonies from other offenses was part of a new attitude toward crime. Although earlier law scarcely discriminated between the various wrongs that people inflict on one another and employed compensations as a remedy for them all, *Glanville* drew a clear line between criminal and civil

<sup>60</sup> *Regula S. Benedicta*, trans. Francis A. Gasquet (London, 1910), Prologue, cap. 7; and David Knowles, *From Pachomius to Ignatius* (Oxford, 1966), 74.

<sup>61</sup> Bernard, *Sermo super cantica*, 20 V, 5-6, as quoted in Southern, *Making of the Middle Ages*, 233; and Moore, "Love in Twelfth-Century France," 433-34. The shifting attitude toward God is also shown by the dwindling of the motif of fear in monastic literature despite the heavy emphasis on fear in the Benedictine Rule. For example, William of St. Thierry mentions fear only three times in the Golden Epistle.

<sup>62</sup> Naomi Hurnard, *The King's Pardon for Homicide Before A.D. 1307* (Oxford, 1969); and Pollock and Maitland, *English Common Law*, 2: 464-69. Also see Kean, "Criminal Liability of Children"; and J. M. Kaye, "The Early History of Murder and Manslaughter," *Law Quarterly Review*, 83 (1967): 365-95, 569-601.



actions; the former were considered an affront to society as a whole rather than to the victim and his kin. This idea found its procedural expression as early as 1166 in the jury of presentment, in which the community became the accuser. In the same period the rights of the kin disappeared so completely that a modern statute was necessary to restore to relatives the opportunity to sue for damages. As these reforms were carried out by the royal government, the arbitrary will of the king found in Germanic law itself came to be bound by procedure. Magna Carta set forth in part what the king's subjects believed these restrictions were—or ought to have been—in the thirteenth century: due process, reasonable amercements, judgment by peers, and justice for all free Englishmen. This process reached its logical conclusion in 1234 when Henry III outlawed Hubert de Burgh. The royal council declared this action null as contrary to the law of the land and ordered Hubert restored to the grace of the king. The king had lost the right to declare his own *inimicus* and to bestow his grace where he chose. Law, though administered in the king's name, was now the possession of the whole community.<sup>63</sup>

IN A PAPER DEALING WITH THE HISTORICAL STUDY OF MENTALITIES, Jacques Le Goff has cited a story of Gregory the Great (ca. 590–600) concerning a monk of his abbey who on his deathbed admitted to having hidden three gold coins in violation of the rule. When informed of this, Gregory ordered that the monk be left to die alone without any consoling word so that the terrified monk would be purged of his sin and his death in anguish would serve as an example to the other monks. “Why,” Le Goff asked, “did not this abbot, who was cultivated and instructed as much as one could then be, go to the bedside of the dying sinner to open for him the door of heaven by confession and contrition? The idea forced itself on Gregory that one must pay for one's sin by exterior signs: an ignominious death and burial (the body was thrown on the dunghill). Barbarian practice (brought by the Goths or revived from ancient psychic depths?) prevailed over the rule. Mentality conquered doctrine.”<sup>64</sup>

The mentality of which Le Goff has spoken (and which he has declined to define) can be understood by reference to cognitive theories of moral reasoning. The novelty of the twelfth century in the history of medieval mentalities lies in the sudden predominance of cognitive structures that were different from those of the early Middle Ages. Evidence drawn from a variety of sources—particularly law, political theory, philosophy, theology, and religious practice—supports this theory. Specialists in the history of these areas of thought have, of course, suggested other explanations for the trends examined here, often pointing to new institutions or rediscovered texts as decisive. But there is also the question of why the people of the twelfth century created such institutions or found those particular texts so exciting. Even when these

<sup>63</sup> Pollock and Maitland, *English Common Law*, 2: 458–59, 477, 581.

<sup>64</sup> Le Goff, “Les mentalités,” in *Faire de l'histoire*, ed. Jacques Le Goff and Pierre Nora, 3 (Paris, 1974): 81.



more particularist approaches seem persuasive individually, moreover, they have the effect of multiplying the causes of what appears in broader perspective as a single historical movement. Despite the many new questions it raises, the present hypothesis does at least satisfy the traditional criterion of economy.

Probably the most disturbing—a colleague insists the correct adjective is “creepy”—aspect of this theory is the evident parallel between the evolution of medieval culture as a whole and the ontogeny of thought in modern children. It should, however, be emphasized that I am not arguing here that there is a natural tendency for societies to evolve or “progress”; indeed, it is likely that an examination of the history of late Roman culture might show a reverse trend—from communitarian to authoritarian conceptions of morality. Nor do I assume that societies possess a consciousness which is somehow similar to that of an individual and capable of maturing. Psychological theories can be applied with confidence only to individuals, and for this reason the term “mentalities” only indicates the beliefs and ways of reasoning shared by actual people living at a given time. Although indirect evidence such as law codes has, of necessity, been employed, the objective has been to understand the consciousness of individuals. Through such evidence one can see differences between the reasoning used by the contemporaries of Odo or Gregory the Great and that used by the contemporaries of Bernard or Abelard.

It could be objected that, by so limiting the content of “mentalities,” the elaborate psychological theory demonstrates little more than a statement of the predominance or statistical frequency of certain modes of thought—without really having any statistics. There is some truth in this objection: obviously not everyone in the ninth or twelfth century reasoned in the same way, any more than everyone today reasons in the same way. It is, however, hard to see what reality an alternative, more abstract definition would describe. Societies are merely aggregates of individuals. The members of a society interact not with impersonal agencies but with each other in a variety of roles, and the rules governing those roles take on meaning only when they are incorporated into an individual’s cognitive structure. Monasticism, for example, did not exist in the abstract; it existed only through the actions and beliefs of individuals, and, when the consciousness of those individuals changed over time, monasticism changed as well. Usually, of course, one generation through its interactions with the next educates it into the same ways of thinking, just as the intellectuals and leaders of the twelfth century—by the institutions they created, the questions they posed, and the students they taught—assured that their concerns would be those of subsequent generations. For this very reason changes in mentalities are extremely rare—and extremely important.

There are, to be sure, several directions for further research. To begin with, an effort must be made to disentangle the connections between social and cultural change in the eleventh and twelfth centuries. Cognitive psychology

should prove to be a helpful tool for this project, since it tries to analyze the way people learn to think from their daily experiences. In addition, nearly every point that has been discussed is susceptible to fuller treatment, and more attention could also be paid to exceptions within each period. The interpretation also ought to be carried forward to the thirteenth century and the later Middle Ages, a period in which concern for maintaining institutions (Kohlberg's stage four) becomes increasingly apparent; since court records and other sources become more abundant after 1200, it may even be possible to investigate the moral attitudes of classes outside the elite.<sup>65</sup> Nor am I unaware that political attitudes and behavior can be brought within the scope of this interpretation.

If research confirms the hypothesis sketched above, our understanding of the Middle Ages will be affected in a number of important ways. First, it will establish a basis for comparing early medieval Europe with other primitive societies. The comparison has been made before on specific grounds: Maine has pointed out the similarity of Anglo-Saxon and other systems of "ancient law" (there are still some societies in which drivers of automobiles involved in fatal accidents are put to death), and Duby has recently interpreted the early medieval economy by analogy to gift-giving economies.<sup>66</sup> The link between these different phenomena may be the prelogical thought described by Piaget, which is common to all humans but which modern Westerners usually abandon in the process of reaching adulthood.<sup>67</sup>

Cognitive psychology also has important implications for studying the relationship between the individual and society. In children the valuing of emotions and interiority grows out of an increasing awareness of one's obligations to others, and Colin Morris has shown that individualism in the twelfth century was closely linked to a heightened awareness of friendship and love. Morris has stumbled, however, in trying to argue that individualism grew out of an alienation from society.<sup>68</sup> In fact, the twelfth and thirteenth centuries are noteworthy for the concern shown about society as a whole, a theme that runs equally through canonists, legists, political theorists, and—if one can make the leap—the barons of Henry III.<sup>69</sup> The influence of Aristotle's *Politics* might also be reconsidered in this perspective, since, as Gaines Post has shown, the naturalism of society was already recognized in the twelfth century.<sup>70</sup> The

<sup>65</sup> For an interesting example of this kind of research, see Thomas A. Green, "Societal Concepts of Criminal Liability for Homicide in Mediaeval England," *Speculum*, 47 (1972): 669–94. Green has shown that, whereas the law put strict limits on the defense of self-defense against a charge of murder, requiring the defendant to show that his assailant tried to kill him and that there was no escape other than to slay him, juries tended to convict solely on the basis of premeditation. When necessary juries even fabricated evidence to secure an acquittal under the rules of law. This attitude (Kohlberg's stage three?) seems greatly to have exasperated the (stage four?) judges.

<sup>66</sup> See G. Duby, *The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century*, trans. H. B. Clarke (London, 1974).

<sup>67</sup> Research on this point has barely begun, but see Piaget, *Structuralism*, trans. C. Maschler (New York, 1971), 114–19.

<sup>68</sup> Morris, *Discovery of the Individual*, 121–38.

<sup>69</sup> See, for example, Chodorow, *Christian Political Theory and Church Politics*; and, more generally, Pierre Michaud-Quantin, *Universitas: Expressions du mouvement communautaire dans le Moyen Age latin* (Paris, 1970).

<sup>70</sup> Post, "The Naturalness of Society and the State," in *Studies in Medieval Legal Theory: Public Law and the State*, 110–1322 (Princeton, 1964), 494–591. For a contrary opinion see the works of Walter Ullmann. He has

*Politics* was important not because it brought new ideas but because it provided a framework for medieval thinkers to discuss ideas in which they already were deeply interested.

In general, then, focusing on logical structure helps us to realize the dimensions of the break that began in the eleventh and twelfth centuries. “Renaissance” is too weak a word to characterize this shift, which comprised much more than a return to antique canons of style or the recovery of classical texts. Indeed, the interpretation and mastery of these texts was possible only because of the shifts in moral consciousness that raised in a new way the question of an individual’s duty to his or her companions and society. A better parallel than the Italian Renaissance is the Greece of the fifth century B.C., when questions of ethics, politics, and science were also discussed in a radically new way.<sup>71</sup> Admittedly, the men of medieval Europe went as fast and far as they did in large part because they could build on Greek and Roman foundations—Aristotle, for example, provided ready-made criteria for logical analysis that otherwise would have had to be created anew. But this should not obscure the unique accomplishments of twelfth-century thinkers who, motivated by a desire for justice and knowledge, reconstructed ancient ideals in an original and lasting way.

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opposed “ascending” and “descending” themes of government; see, e.g., his *Principles of Government and Politics in the Middle Ages* (New York, 1966). As Francis Oakley has shown, Ullmann considered the ascending, populist theme more basic and treated the descending, theocratic theme as a Christian aberration redressed, in part, by Aristotle: “Celestial Hierarchies Revisited: Walter Ullmann’s Vision of Medieval Politics,” *Past & Present*, no. 60 (1973): 3–48. Ullmann’s epistemology, of course, reverses Piaget’s.

<sup>71</sup> A. C. Goodson has kindly informed me that in classical Greek literature the earliest moral judgment based on intention of which he knows is in *Oedipus at Colonus*, Sophocles’ last play, in which the Athenians agree Oedipus was innocent because he acted unknowingly.

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# The Character of Erasmus

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AND

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THE CHARACTER OF DESIDERIUS ERASMUS OF ROTTERDAM (ca. 1467–1536) has over the years been the object of widely divergent opinion. He is often presented as the gentle, but insistent, voice of moderation and toleration amid Reformation polemics, a view popularized by recent generations of “liberals” and skeptics, but modified by current scholarship. Yet, in his own lifetime and for centuries thereafter, conservative Catholics vilified him as the sower of bitter discord and the followers of Luther denounced him as a cowardly traitor to the Gospel.<sup>1</sup> These varying views are due not only to differing theological and philosophical perspectives, but also to the apparently contradictory evidence to be found in Erasmus’ writings and actions. With the help of psychology a few explicit attempts have been made to understand the complexities of Erasmus’ character.<sup>2</sup> Unfortunately, their findings are limited

We are grateful to William J. Bouwsma, Myron P. Gilmore, John W. O’Malley, Thomas and Catherine West, and others for having suggested a number of helpful revisions. Whatever deficiencies remain should be attributed solely to the authors.

<sup>1</sup> For a bibliographical survey of some of the vast literature on Erasmus, see Andreas Flitner, *Erasmus im Urteil seiner Nachwelt: Das literarische Erasmusbild von Beatus Rhenanus bis zur Jean Leclerc* (Tübingen, 1952); Bruce E. Mansfield, “Erasmus in the Nineteenth Century: The Liberal Tradition,” *Studies in the Renaissance*, 15 (1968): 193–219; Preserved Smith, *Erasmus: A Study of His Life, Ideals, and Place in History* (New York, 1923), 421–41; Jean-Claude Margolin, *Douze années de la bibliographie érasmienne (1950–1961)* (Paris, 1963); and Rudolf Padberg, *Personal Humanismus—Das Bildungsverständnis der Erasmus von Rotterdam und seine Bedeutung für die Gegenwart: Ein Beitrag zur Revision des Humboldtschen Bildungsideals* (Paderborn, 1964), 21–38. For some of Erasmus’ principal modern biographers, see Augustin Renaudet, “Érasme, sa vie et son oeuvre jusqu’en 1517,” *Revue historique*, 111 (1912): 225–62 and 112 (1913): 241–74, *Érasme, sa pensée religieuse et son action d’après sa correspondance, 1518–1521* (Paris, 1926), *Études érasmienne, 1521–1529* (Paris, 1939), and *Érasme et L’Italie* (Geneva, 1954); Smith, *Erasmus*; Johan Huizinga, *Erasmus*, Eng. trans. F. Hopmen (New York, 1924); Albert Hyma, *The Youth of Erasmus* (Ann Arbor, 1930; rev. ed., 1968), and *The Life of Desiderius Erasmus* (Assen, 1972); Margaret Mann Phillips, *Erasmus and the Northern Renaissance* (London, 1949); Louis Bouyer, *Autour d’Érasme: Études sur le Christianisme des humanistes catholiques* (Paris, 1955); Ronald H. Bainton, *Erasmus of Christendom* (New York, 1969); and James Tracy, *Erasmus: The Growth of a Mind* (Geneva, 1972).

<sup>2</sup> The most important remains the three chapters of Huizinga’s *Erasmus* which presents a psychologically perceptive, if overly negative, picture of an unhappy, self-centered, spiteful, ambivalent, and lonely man. The physician John Joseph Mangan portrays a sickly man of great literary talent whose personality was unimpressive: supremely egotistic, neurasthenic, morbidly sensitive, volatile, variable, and vacillating, injudicious, irritable, and querulous, yet “always . . . a baffling but interesting character”; *Life, Character and Influence of Desiderius Erasmus of Rotterdam: Derived from a Study of His Works and Correspondence*, 1 (New York, 1927): xi. Victor W. D. Schenk claims that Erasmus was a volatile neurotic, latent homosexual, hypochondriac, and psychasthenic; “Erasmus’ Character and Diseases,” *Nederlandsch tijdschrift voor gen-*

to description rather than analysis, formulated in an outmoded terminology, presented only in passing, or marred by inaccuracies when authors venture outside their areas of competence. Furthermore, these studies do not integrate their valuable insights into a coherent psychological pattern, nor do they help to explain the course of Erasmus' career. The following collaborative study of a psychiatrist and a historian seeks to resolve the seeming contradictions of Erasmus' character and arrive at a psychoanalytically intelligible configuration consistent with the historical data.

MOST OF OUR SCANTY INFORMATION ABOUT the earliest years of Erasmus' life comes from the *Compendium Vitae*, a purportedly autobiographical piece whose covering letter is dated 1524. Although its authorship has been contested, a number of the events it records find confirmation in other documents of the period, and the pattern of Erasmus' adult attitudes and behavior is psychologically consistent with the circumstances of childhood described in this *Compendium*.<sup>3</sup> Erasmus was born on October 27/28, probably in 1467,<sup>4</sup> of an unlawful and, "as he feared, a sacrilegious union." His mother was a widow, and the father was unmarried and probably already a cleric. According to the *Compendium* the father was named Gerard. There is some confusion about his identity because a papal brief referred to Erasmus as the son of Rogerius.

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eeskunde, 91 (March 22, 1947): 702-08. The recent publication by Harry S. May, *The Tragedy of Erasmus: A Psychohistoric Approach* (St. Charles, Mo., 1975) attempts to find the psychological roots of Erasmus' anti-Semitism. And for a blistering review of this work, see Wallace K. Ferguson, in the *American Historical Review*, 82 (1977): 376-77. The secondary literature on Erasmus and his times is already enormous. We do not attempt to list the many works that have influenced our thinking on Erasmus, but cite in the appropriate places a few that were important for our understanding of a particular issue or event. Whenever possible and desirable we have cited primary rather than secondary sources. Preference has been given here to Erasmus' letters since they reveal in a special way his thoughts, feelings, and ways of dealing with friends, acquaintances, religious superiors, patrons, and opponents. References to his other writings cite the standard edition of Jean Leclerc: Desiderius Erasmus Roterodami, *Opera omnia emendatiora et auctiora ad optimas editiones, praecipue quas ipse Erasmus postremo curavit, summa fide exacta doctorumque virorum notis illustrata* (Lugduni Batavorum: Cura et impensis Petri Vander Aa, 1703-06); hereafter this edition is cited as LB. The more recent but as yet unfinished critical edition *Opera omnia Desiderii Erasmi Roterodami recognita et adnotatione critica instructa notisque illustrata* (Amsterdam, 1969-) is hereafter cited by its standard abbreviation ASD.

<sup>3</sup> For a critical edition of this work, see Percy Stafford Allen, ed., *Opus Epistolarum Desiderii Erasmi Roterodami*, 12 vols. (Oxford, 1906-48), 1: 47-52; hereafter this edition of correspondence, notes, and indexes published with the assistance of Helen Mary Allen *et al.* will be cited as *EE*, an abbreviation of its shorter title *Erasmi Epistolae*. References to the individual letters of Erasmus cite the arabic number of the appropriate letter (no.) and relevant lines (ll.); all other references to material in Allen's collection are by volume and page number (e.g., 1:47-52). For a treatment of the arguments surrounding the authorship of the *Compendium Vitae*, see Francis Morgan Nichols, *The Epistles of Erasmus*, 1 (New York, 1901): xlvii-li, 2-3; *EE*, 1: 575-78, app. 1; Roland Crahay, "Recherches sur le *Compendium Vitae* attribué à Erasme," *Bibliothèque d'Humanisme et Renaissance*, 6 (1939): 7-19, 135-53; Tracy, *Erasmus*, 21, n. 2; Huizinga, *Erasmus*, 5; and Hyma, *The Youth of Erasmus*, 52. Our discussion of the character of Erasmus does not rest on or derive from the circumstances recounted in the more than likely romanticized *Compendium Vitae*; rather our reconstruction rests on more specific evidence derived from Erasmus' life, patterns of behavior, frequently expressed attitudes and sentiments, letters, and writings of various sorts. The *Compendium Vitae*, with its apparent coloration of the story of Erasmus' origins, merely blends into this overall picture. It is not essential to the argument of our analysis.

<sup>4</sup> For a detailed discussion of the problems involved in dating Erasmus' birth, see A. C. F. Koch, *The Year of Erasmus' Birth and Other Contributions to the Chronology of His Life*, trans. E. Franco (Utrecht, 1969), 1-7, 39-44; and the annotated references in Bainton, *Erasmus of Christendom*, 27, n. 2.

Erasmus called his brother Peter Gerard. Some historians suggest that his father's name may have been Roger Gerard.<sup>5</sup> The *Compendium* recounts that the father had, with the expectation of marriage, an affair with Margaret, the daughter of a physician. They may even have been secretly betrothed, but Gerard's family staunchly opposed the union. Nine brothers, all married, insisted that one of the family be consecrated to God. In the face of this opposition, Gerard fled, leaving his intended wife with child. Learning that Gerard was in Rome, his parents wrote him that the girl he had planned to marry was dead. He believed this, and in his grief became a priest and applied himself to religion. Only after returning home did he discover the deception.<sup>6</sup>

Erasmus probably knew that this story was an apparent if convenient idealization, inaccurate in detail. More likely, his father was already a cleric at the time of the affair, and the conception was not the result of impatient passion, but of an irregular union of several years standing—Erasmus' brother had been born three years earlier.<sup>7</sup> If Erasmus was the author of the *Compendium*, he would naturally have wished to obliterate the sordidness of his origins by creating such a romanticized myth. The "family romance," in which the child fantasizes that he is the offspring not of his present parents but of some more important or powerful persons, is a familiar phenomenon. The fantasied parents possess the idealized characteristics with which the child had endowed his parents during the period of infantile narcissism. The child must somehow rationalize the discrepancy between this romanticized infantile image and his parents as they actually are.<sup>8</sup> The author of the *Compendium* seems to have idealized not only the circumstances of Erasmus' conception but also the father's subsequent absence. Gerard was depicted as the helpless victim of the imposed wishes of his family. In desperation he secretly fled to Rome, where he "lived like a young man," applied himself to the liberal arts, thereby gaining an excellent knowledge of Latin and Greek, and was forced by grief and cruel circumstances to become a priest. After discovering the deception, the father honorably refrained from touching Erasmus' mother again and provided for the education and support of their child.<sup>9</sup> The *Compendium* thus seems to absolve the father of blame for both Erasmus' illegitimacy and the father's continued absence.

The early experiences apparently had a significant influence on Erasmus'

<sup>5</sup> *EE*, no. 517, ll. 7–8; no. 187a, ll. 4–5; *EE*, 1: 578, app. 1; and Huizinga, *Erasmus*, 6. For arguments suggesting Gerard as his father's baptismal name and Rogerius as his mother's surname, see Hyma, *The Youth of Erasmus*, 55–56; and Nichols, *Epistles of Erasmus*, 37–39.

<sup>6</sup> *EE*, 1: 47–48, *Compendium*, ll. 1–28.

<sup>7</sup> Bainton, *Erasmus of Christendom*, 8; Huizinga, *Erasmus*, 5; and Hyma, *The Youth of Erasmus*, 52–54. In another work Hyma cites a local tradition recorded by Loos later in the sixteenth century whereby Erasmus' mother was the housekeeper at Gouda of his priest-father and was sent away to her mother's house in Rotterdam after becoming pregnant; see *Life of Desiderius Erasmus*, 10–12. If Erasmus' mother were a widow as claimed in the papal brief, Peter could also have been his half brother; see *EE*, no. 187a, l. 5. The question of the immediate sources of the account in the *Compendium* is open to speculation.

<sup>8</sup> Sigmund Freud, *Family Romances* (1909), in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, trans. James Strachey et al., 24 vols. (London, 1953–74), 9 (1959): 235–41.

<sup>9</sup> *EE*, 1: 47, 48, *Compendium*, ll. 21, 28–31.



development.<sup>10</sup> From the account of the father's absence, we can hypothesize a deep yearning in Erasmus for closeness with his father. Erasmus' inclination toward learning and his dedication to classical literature and to religion suggest a considerable identification with the father.<sup>11</sup> The son's tendency to present himself as the helpless victim of malicious forces is strikingly similar to the portrayal of the father in the *Compendium*. But it also seems reasonable to postulate that this father's remoteness and abandonment of him frustrated Erasmus' longing for closeness and produced a deeper resentment which was repressed only to manifest itself in later life. The *Compendium* reports that he was raised at his grandmother's house, presumably by his own mother. When at the age of nine he went off to school at Deventer where, he stayed until 1483, his mother followed along to watch over and care for him.<sup>12</sup> She was probably a devoted and doting mother, upon whom Erasmus became all the more dependent owing to his father's absence. Children who are forced into excessive dependence on parental figures tend to develop ambivalence toward them.<sup>13</sup> If Erasmus conformed to the model, he both identified with the mother and resented her. The patterns of masculine and feminine identification and the influences which give rise to them are important for the analysis of character development. A very likely consequence of the child's unsatisfied desire to be loved by his father is an identification with the mother who is the

<sup>10</sup> In order to keep in focus the delicate interplay between historical judgment and clinical interpretation, it is useful to quote Erik H. Erikson's comments on the available data concerning Luther's childhood. He observes, "Except for bits of often questionable amplification here or there, and some diligent background study by the biographers . . . these are all the facts we have. If any determining insight had to be drawn from this material alone, it would be better not to begin. But a clinician's training permits, and in fact forces, him to recognize major trends even where the facts are not all available; at any point in a treatment he can and must be able to make meaningful predictions as to what will prove to have happened; and he must be able to sift even questionable sources in such a way that a coherent predictive hypothesis emerges. The proof of the validity of this approach lies in everyday psychoanalytic work, in the way that a whole episode, a whole life period, or even a whole life trend is gradually clarified in therapeutic crises leading to decisive advances or setbacks sufficiently circumscribed to suggest future strategies. In biography, the validity of any relevant theme can only lie in its crucial recurrence in a man's development, and in its relevance to the balance sheet of his victories and defeats." See Erikson, *Young Man Luther: A Study in Psychoanalysis and History* (New York, 1958; reprint ed., 1962), 50. For scholarly critiques of this work, see Roger A. Johnson, ed., *Psychohistory and Religion: The Case of "Young Man Luther"* (Philadelphia, 1977); a bibliography of other reviews is given on pages 197-98.

<sup>11</sup> This description of his father's interest in classics comes from the *Compendium*. Corroborating evidence is meager. His father, apparently a skilled copyist, is known to have possessed a number of books, the sale of which Erasmus urged after his father's death; see *EE*, no. 1, ll. 7-15. The elements of paternal identification which appear in the *Compendium* are reflected in Erasmus' experience. The link between Erasmus and the *Compendium* remains open to question, but the psychological force of the argument points in the direction of such a paternal identification.

<sup>12</sup> *EE*, 1: 47-48, *Compendium*, ll. 18-19, 32-34. Explicit references to his mother are rare in Erasmus' writings. This characterization is consistent with the slender references to her and is congruent with Erasmus' character. The nature of a child's relationship to his parents can often be clinically inferred from the patterns of his adult behavior. See Sigmund Freud, "Some Character-Types Met Within Psychoanalytic Work" (1916), in *Standard Edition*, 14 (1957): 309-33; Sigmund Freud, "The Ego and the Id" (1923), in *Standard Edition*, 19 (1961): 1-66; and H. H. Tartakoff, "The Normal Personality in Our Culture and the Nobel Prize Complex," in R. M. Loewenstein et al., eds., *Psychoanalysis: A General Psychology* (New York, 1966), 252-72.

<sup>13</sup> M. S. Mahler, "On Human Symbiosis and the Vicissitudes of Individuation," *Journal of the American Psychoanalytic Association*, 15 (1967): 740-63; and Gregory Rochlin, *Griefs and Discontents: The Forces of Change* (Boston, 1965).

object of the father's romanticized love. This can serve as the basis for homosexual yearnings for closeness with the father.<sup>14</sup>

Erasmus was sensitive on the issue of illegitimacy. Occasional remarks made in later years show that his tainted origins affected him. He noted that children known for their virtue do not usually spring from evil parents. Repeating the commonplace wisdom of humanists, he also insisted that only personal sin makes one base, while the practice of virtue produces nobility. Since God does not impute the sins of parents to their offspring, children should not be treated as though they share in the parents' sins.<sup>15</sup> Erasmus' illegitimacy did, however, color the rest of his experience. According to the psychology of "exceptions," originally discussed by Freud, persons born with some physical defect feel that because of the wrong done to them at birth they are special individuals free of the constraints placed on ordinary men.<sup>16</sup> Illegitimacy was Erasmus' congenital defect. We can infer that it made him feel special and privileged. The efforts of a doting mother would have contributed to these feelings. The continued absence of his father must have extended the initial sense of deprivation, for which Erasmus' narcissism may have demanded compensation in the form of maternal attention. A suggestion that Erasmus may have succeeded in gaining this attention and later reflected on its harmful effects is found in a letter to Severin Boner, written when Boner sent his young son away to be educated. Erasmus declared that the child would be better off without the excessive kisses of his mother and the hugs of nurses and would spend his time in more useful pursuits.<sup>17</sup>

Erasmus described in dismal terms his fate after the premature death of both parents at the time of the plague.<sup>18</sup> The three guardians placed over him by his father's will consulted a "haughty" Franciscan, whose "sense of

<sup>14</sup> Freud, "The Ego and the Id," 1-66. We do not wish to overstate the argument for homosexual elements in Erasmus' personality. But it is consistent with the data. Remoteness from the father and close dependence on the mother is common among male homosexuals.

<sup>15</sup> Erasmus, *Moriae Encomium* (1509/11) (LB), 4: 429c and *Liber de sancienda ecclesiae concordia* [equals LB's *De amabili ecclesiae concordia liber: Enarratio psalmi LXXXIII*, 1533] (LB), 5: 474a.

<sup>16</sup> Freud, "Some Character-Types Met within Psychoanalytic Work," 309-33. The "exceptions" were Freud's first attempt to describe a form of character pathology based on narcissistic elements. The concept of narcissism was advanced in his 1914 paper, "On Narcissism: An Introduction," in *Standard Edition*, 14 (1957): 67-102. The pathological aspects of Erasmus' character can best be understood as a form of narcissistic character disorder or narcissistic personality. Not all narcissism is pathological. Healthy and well-integrated narcissism can serve as the basis of positive character traits such as ambition, ideals, values, self-esteem. Narcissism, in a simplified sense, can become pathological either by excess or by defect. The pathological excess of narcissism expresses itself in traits of pride, hypersensitivity, demandingness, a sense of entitlement and specialness, the expectation of privilege, special consideration, and the desire for recognition beyond what has been merited. The pathological defect of narcissism appears in traits of depression, feelings of inadequacy and lack of worth, shame, envy, jealousy, and a sense of vulnerability and impotence. The "exceptions" would then be a typical form of narcissistic character pathology by excess. In Erasmus, as in most narcissistic personalities, the elements were mixed.

<sup>17</sup> *EE*, no. 2533, ll. 66-70; Erasmus urged, however, a close physical and affectionate relationship between a mother and her baby during the child's earliest years. See *Colloquia*, "Puerpera" (1526), ed. Léon-E. Halkin et al. (ASD), 1, pt. 3 (1972): ll. 161-91, 514-51, pp. 457-58, 467-68.

<sup>18</sup> The age of Erasmus at the death of his father varies between fifteen and eighteen years depending on the date of his birth. He left Deventer after his mother died, probably in the plague of 1483. His father died soon afterwards. See *EE*, 1: 582, app. 2; Koch, *Year of Erasmus' Birth*, 27-28; and Regnerus R. Post, *The Modern Devotion: Confrontation with Reformation and Humanism*, trans. Mary Foran, vol. 3 of *Studies in Medieval and Renaissance Thought*, ed. Heiko A. Oberman et al. (Leiden, 1968), 351, 658.

judgment was no better than that of common folk” and who thought “that he was sacrificing to God a most pleasing victim whenever he dedicated one of his pupils to the monastic life.” On his advice, the guardians sent Erasmus to ’s-Hertogenbosch rather than to a university. As he noted bitterly, “They feared the university, for they had decided to rear the boy for the religious life.” At ’s-Hertogenbosch Erasmus lived in a house of the Brethren of the Common Life, where, he later claimed, his teacher was bent on ruining good natural talents and instilling monasticism. His guardians also put pressure on him to enter the religious life. Others were enlisted in an effort “to entice, intimidate, and sway his unsteady mind.” For a time Erasmus resisted these pressures by pleading his lack of knowledge about himself and the world he would renounce. In frustration, one of his guardians threatened to resign his charge. Erasmus’ older brother Peter, upon whom he probably became dependent, was subjected to the same pressure. When his brother yielded, Erasmus felt betrayed. Deserted by Peter and weakened in health, Erasmus finally gave in to the wishes of his guardians and entered the monastery of the Augustinian Canons at Steyn, where he took vows in 1488. He later stated that he had reached this decision only after having visited the monastery and having found there an old school chum from Deventer who pointed out to him some of the advantages of monasticism—particularly “the holiest way of life, the abundance of books, leisure, peace, and angelic companionship.”<sup>19</sup>

In later life Erasmus remembered the transfer to Steyn as something forced on him. Yet his letters from there do not indicate a deep-seated aversion to monastic life. However much he may have chafed against its restrictions, he probably still found much in the monastic setting that was congenial, especially the leisure and relative freedom to pursue his intellectual interests. During this period he penned an encomium on monastic life which he later excused as a favor for a friend who wished to persuade a nephew to enter the monastery—a naive rationalization at best.<sup>20</sup> In later years he claimed that the members of his monastic community were intellectually dull—“naturally slow in comprehension, partial fools, untouched by the Muses, and greater lovers of their bellies than of learning”—and that anyone who showed unusual talent or capacity for learning was squelched and his talent prevented from blooming. Erasmus buried himself in his studies, for “by means of literary pursuits he escaped the tedium of his captivity.” Indeed, if the Bishop of Cambrai had not rescued him from the monastery in 1493, there was danger that he would have succumbed to temptation and that “his remarkable talents would have rotted away through idleness, allurements, and drinking parties.”<sup>21</sup>

Erasmus tended to look back with scorn on his early education at Deventer, ’s-Hertogenbosch, and Steyn. His gloomy accounts, however, need qualification. While he gives the impression of having been in large part a struggling,

<sup>19</sup> *EE*, 1: 49–50, ll. 50–86; and no. 447, ll. 81, 86, 92–94, 282–84.

<sup>20</sup> Hyma, *The Youth of Erasmus*, 167–81.

<sup>21</sup> *EE*, no. 447, ll. 373–77, 451–57.

self-made scholar, he probably owed to his teachers and peers at Deventer more than he was ready to admit. The noted grammarian John Xinthen, one of the Brethren of the Common Life, who supervised the studies of and gave supplementary lessons to those staying in the student hostel there, affectionately praised the young Erasmus and encouraged him in his Latin studies, predicting that some day he would reach the summit of learning. Alexander Hegius—friend of Rudolf Agricola, eventual collaborator with Xinthen on a grammar commentary, and headmaster of the school at Deventer—provided Erasmus with a model of the Northern humanist. Hegius exhorted his students in the new learning during Erasmus' final period at the school. While esteeming this schoolmaster, Erasmus claimed in later life that he owed him little. The youthful Erasmus was not the only pupil at Deventer interested in classics. We know of two other students, the cousins Cornelius Gerards and William Hermans, who shared with him their enthusiasm for the classics and later joined him at Steyn in common literary pursuits.<sup>22</sup>

Erasmus' attacks on his former teachers and their methods were not without some basis. His allegation that the Brethren of Common Life, who supervised his studies at Deventer and 's-Hertogenbosch, systematically "tamed" the lively spirits of talented youths in order to mold them for monastic life is partially confirmed by evidence found in other sources.<sup>23</sup> His claims that schooling was made a painful experience, that what was learned had later to be unlearned, and that his teachers were less skilled in Latin than their student probably also contained elements of truth. The training received by the youthful Erasmus had centered on medieval Latin grammars with rules based on logic rather than usage. His propensity for seeing himself as oppressed, however, probably colored his accusations against his teachers. That he exaggerated his plight is suggested by the leadership exerted by some of the Brethren in popularizing a scholarly study of Latin.<sup>24</sup>

What drew the brilliant student Erasmus to classical studies, rather than to rival disciplines such as scholastic theology and canon law, is open to speculation. The initial attraction may have been related to an effort to identify with his absent, classically trained father and to win the attention and affection of his Latin teachers, the father figures most available to him. He was no doubt encouraged in his studies by the exceptional success which greeted his efforts, as he was said to have surpassed all his classmates in his knowledge of Latin.<sup>25</sup> Classical studies also lifted him from his sordid origins and dull surroundings to the great minds of the past and eventually to the learned circles of his own day with their aristocratic patrons. As his comprehension of the humanist culture matured, he probably found a remarkable symmetry between many of the major themes it espoused and his own deeper needs: a flight from this

<sup>22</sup> *Ibid.*, 1: 48, *Compendium*, ll. 39–40 and n.; 1: 57, no. IV, ll. 11–16, 20–27; and Post, *The Modern Devotion*, 576–79, 659, 674–75.

<sup>23</sup> *EE*, 1: 49, *Compendium*, ll. 53–56; no. 447, ll. 103–07; and Post, *The Modern Devotion*, 254–55, 396–97, 660.

<sup>24</sup> *EE*, 1: 48, *Compendium*, ll. 34–36; no. 447, ll. 118–20; no. 2584, ll. 25–29; Post, *The Modern Devotion*, 577, 658–59; and Hyma, *Life of Desiderius Erasmus*, 18–21.

<sup>25</sup> *EE*, 1: 57, no. IV, ll. 16–18.

decadent world to an idyllic past unlike his own; a refined language that allowed him to cloak his own emotions; a quest for peace and concord that provided a defense against his own sense of vulnerability; rhetorical ambiguities that abhorred the clear distinctions, stark assertions, and univocal truths characteristic of scholastic theology (and to a lesser degree of canon law) and that matched the ambiguities of his own personality; a belief that the path of wisdom lay in rhetorical eloquence rather than in the rational analysis of the scholastics, which he found too difficult; and membership in an elite international community of scholars, which allowed him to escape his origins, avoid entangling loyalties, and sink few roots.<sup>26</sup> Erasmus came to appropriate this culture so passionately that he was hailed as its embodiment, the prince of humanists.

One of the more interesting incidents of his years at Steyn was Erasmus' friendship with Servatius Roger. The letters to Servatius reveal an aspect of Erasmus' character that rarely appeared elsewhere: a certain feminine sensitivity and a sentimental need for friendship. Erasmus begged for a return of affection, protesting that Servatius was always in his heart, his only hope, his other self, the solace of his life. "I shall never stop loving you," he finally affirmed.<sup>27</sup> This outburst of sentimental affection seems to reveal a latent homosexuality, which may in part reflect the persistent yearning for his lost father. Servatius, we can note, had the same name Roger as did Erasmus' father. But this sharing of names is not at all necessary. Attempts have been made to excuse this excessive affection by references to models of intimate friendship found in Erasmus' readings of classical literature, to the sentimental friendships then in vogue within and without the monastery walls, to the likely response of one debarred from love and placed in a crude and cold environment, and to a monastic tradition of rhetorical letters. Although such literary expressions of affection were commonplace among Renaissance humanists, Erasmus seldom used them. Apparently Servatius did not reciprocate his effusive and exclusive affection. Erasmus learned to be more restrained; only rarely did he again give expression to his tender feelings. Instead, he became the guarded, yet witty and urbane, humanist who engaged others on a primarily intellectual level. Emotions were measured before being expressed, and it was increasingly the litany of his sorrows that he chose to share.<sup>28</sup>

<sup>26</sup> John W. O'Malley, "Erasmus and Luther: Continuity and Discontinuity as Key to Their Conflict," *Sixteenth Century Journal*, 5 (1974): 47-65.

<sup>27</sup> Of his letters to Servatius, see especially *EE*, no. 7, all, and no. 8, ll. 61-68.

<sup>28</sup> Huizinga, *Erasmus*, 11-12; and D. F. S. Thomson, "Erasmus as Poet in the Context of Northern Humanism," *De Gulden Passer*, 47 (1969): 192-97. Hyma sees these letters as expressions of neurotic affection; *The Youth of Erasmus*, 160-62. While the expression of tender affection was common enough in literary, humanistic circles of the period for it to be regarded more or less as a stylistic convention, this does not contradict the interpretation of such utterances as expressing basically homosexual impulses. One of the points at issue between the historical perspective and the psychoanalytic perspective is the extent to which patterns of behavior and expression may be taken to reflect stylistic conventions or cultural variants as opposed to unconscious psychodynamic motivations. There is no inherent reason, however, why such conventions and cultural expressions should not concurrently express psychodynamically significant and multiply (including unconsciously) determined influences. The historical approach prefers to disregard or,



A year after ordination to the priesthood, Erasmus left the monastery at Steyn to become secretary to Henry of Bergen, bishop of Cambrai and important member of the Burgundian court. Pleased at first with his new job, Erasmus grew despondent when his duties became routine. The bishop lost interest in him, and a projected journey to Rome never materialized. Brought by his secretarial position into the harsh, bustling world of politics, Erasmus—not unlike other court humanists of his day—longed at times for the leisure to study that Steyn had afforded.<sup>29</sup> We can surmise that Erasmus' apparent depression arose in part from his loss of the safe and protecting—we might almost say “maternal”—environment of the monastery and its quiet seclusion. More significant, however, was the loss of the close and meaningful relationships that he had developed, not only with Servatius, but apparently also with William and Cornelius, his companions at Steyn. Erasmus' dejection is understandable in terms of his need to mourn such an important loss. Yet, on another level, an element of mild dissatisfaction, restiveness, disillusionment, dejection, and even bitterness and resentment pervaded much of his career, suggesting that he felt in some basic sense deprived of what he had a right to expect.

With help from his friend James Batt, Erasmus persuaded his employer to send him to Paris to study theology. The parsimony of his patrons and the difficulty of his scholastic studies at Paris gave Erasmus much room for complaint. He objected to scholastic theology's barren and thorny subtleties, to its indifference to stylistic elegance, and to the arrogance and counterfeit lives of its proponents, especially those “pseudoth theologians” who were his Scotist teachers at the Sorbonne.<sup>30</sup> We can grant a significant “kernel of truth” in Erasmus' complaints and still suggest that his hostility and contempt, which persisted over the years, reflected an aspect of Erasmus' narcissism, which found it difficult to acknowledge worth in that which he could not understand or master and hence perceived such an affronting object as hostile.

Erasmus' life at Paris was one of uncertainty and unrest. His health was never very good, and the austerity of his living conditions there weakened it further. He seemed to be continually depressed. He saw himself as an underserving victim, without the support of friends and so crushed by misfortune that he no longer wished to live. While studying at the Sorbonne he had tried to cultivate friendships with the leading local humanists. But these men were not very responsive to his writings. Unfavorable reports about him reached his

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at least, to de-emphasize a more remote or esoteric explanation when more tangible, obvious, common sense and historically verifiable explanations are at hand. The psychoanalytic approach, for its part, prefers to emphasize the psychodynamically relevant aspects of the evidence in addition to other available explanations. Thus, the ascribed historical reasons, however valid, do not abrogate the inherent homosexuality of Erasmus' romanticized effusions. One reason, perhaps, for this frequently encountered tension in viewpoints is the congeniality of overdetermination to the psychological mind in contrast to the historian's preference for linear causality.

<sup>29</sup> *EE*, 1: 57–58, no. IV, ll. 39–63; and Huizinga, *Erasmus*, 16–17.

<sup>30</sup> *EE*, no. 64, ll. 6–92, esp. 45–46, 88–92; and no. 108, ll. 20–31.



episcopal patron in Cambrai, endangering his pension. In the hope of supporting himself, Erasmus increasingly turned to tutoring. One of his pupils in Paris was the young Lord Mountjoy, William Blount, who urged Erasmus to visit England. In early 1499, Erasmus made the trip in Mountjoy's retinue.<sup>31</sup>

The year in England opened up new horizons for Erasmus. Traveling in aristocratic circles, he met men of culture and gladly shared in the diversions of court life. At Oxford he came under the influence of the humanist-theologian John Colet, whose exegesis of Scripture departed from the traditional scholastic method, adopted a neoplatonic framework, and employed philology to uncover theological truths in the texts. This approach appealed to the inclinations and linguistic skill of Erasmus. Colet even urged him to lecture on Scripture. In turning down this offer, Erasmus explained that, while secular studies were beneath his ambitions, he lacked as yet the tools necessary for this new theology.<sup>32</sup> First, he must master Greek, the language of the New Testament and of many of the leading Church Fathers. The linguistic skills he perfected in the following years resulted in critical editions of these early Christian writings. His scholarly interests turned more and more to the field of theology—not the speculative theology he had struggled with at Paris, but the philological exegesis of sacred Christian texts learned from Colet.

The strong impression which Colet made on the Dutch humanist may have been related to Erasmus' probably frustrated, but perduring, desires for closeness to a father whose image matched the reality of Colet. Like Colet, his father was reportedly a priest, a man of classical learning, a student of Greek, and a visitor to Italy—the home of humanism. Erasmus openly sought Colet's affection and years later frankly rejoiced in winning it: "He loves me deeply, as everyone knows, and prefers my company to that of anyone else."<sup>33</sup> While there were antecedents in Erasmus' intellectual development for his eventual interest in both Greek literature and a theology utilizing his linguistic skills, the personal example and encouragement of Colet may have led him to dedicate his talents to this "new theology."

On his return to the Continent in 1500, Erasmus devoted himself to the study of Greek, published a collection of Latin and Greek proverbs known as the *Adagia*, and penned his popular handbook on piety, the *Enchiridion Militis Christiani* (1503). Two years after its publication, Erasmus was once again in England, this time in close association with Thomas More. He stayed in More's home, and together they worked on translations of the Greek satirist Lucian. From 1506–09 Erasmus wandered about Italy, gaining a doctorate in theology at Turin, tutoring at first the sons of the English royal physician, Gian Battista Beorio, and then Alexander Stuart, the youthful archbishop of St. Andrews, publishing an enlarged edition of the *Adagia*, visiting with Italian humanists and their patrons, and securing at Rome the friendship of high

<sup>31</sup> *Ibid.*, 1: 58, no. IV, ll. 63–80; no. 83, ll. 85–100.

<sup>32</sup> *Ibid.*, no. 108, ll. 95–101.

<sup>33</sup> *Ibid.*, no. 107, ll. 37–69; no. 296, ll. 141–42. For a letter implying that Erasmus was one of Colet's closest friends, see *EE*, no. 1211, ll. 371–78.

churchmen and a dispensation to hold benefices. Hopes of preferment with the accession of the young humanist Henry VIII to the throne of England brought Erasmus back across the Alps. On his journey northward he looked forward to visiting again with his good friend Thomas More, the wisest and wittiest of men, whose surname in Greek ironically means "fool." This piece of philology inspired Erasmus to reflect on the role folly plays in human affairs and to compose mentally a declamation on folly in the style of Lucian as a gift for his friend More.<sup>34</sup>

On arriving in England, Erasmus lived in More's household in Bucksbury, near London. Here he found warmth, quiet kindness, and acceptance in a normal family life such as he had never known in his youth. Without his precious books to comfort him (they were still in shipment) Erasmus wrote, within a short time, what must stand as the most uncharacteristic of his works—the *Moriae Encomium* (*Praise of Folly*). It is as if, after years of restraint and repression, all that was capricious, playful, and witty in Erasmus was suddenly given license, and we are allowed to see a part of Erasmus' soul that elsewhere is little visible.

The *Praise of Folly* was more than a satiric commentary on human foibles and delusive self-love; it was a revelation of Erasmus himself, an exquisite and elaborate self-parody. Erasmus—the man of learning, of even pre-eminent learning—made Folly declare "that man is truly the happier who acts the fool in many ways." Or again, "as therefore those professions are the happier which have the greater affinity with Folly, so those people are the happiest by far who have been allowed to abstain absolutely from commerce with all learning and follow nature alone as their guide." It is not learned men, but Folly's followers who are candid and speak the truth, a function not to be despised. "What, moreover, is more praiseworthy than truth?"<sup>35</sup>

The relationship between the mind of Erasmus and the words of Folly has troubled commentators on the *Moriae Encomium*. Literary analyses reveal deliberate ambiguities that at times effectively disguise the author's true views.<sup>36</sup> From another perspective, however, these ambiguities form a consistent pattern explained by Erasmus' personality. In portions of the *Praise of Folly* the more characteristic voice of Erasmus is easily recognizable in its biting cynicism and especially in its excoriating attack upon his favorite targets—the theologians and monks. When the mask of Folly slips, the face of Erasmus is revealed. But even when the mask stays in place, the disguise can still be penetrated. Folly is indeed Erasmus—not the Erasmus usually in evidence, but the Erasmus who, though usually concealed behind a facade of

<sup>34</sup> *Ibid.*, no. 222, ll. 1–24.

<sup>35</sup> Erasmus, *Moriae Encomium* (LB), 4: 440d, 435c, 437e.

<sup>36</sup> A. H. T. Levi, Introduction to Erasmus of Rotterdam, *Praise of Folly and Letter to Martin Dorp*, 1515, trans. Betty Radice (Baltimore, 1971), 15–16. For a careful delineation of the various roles played by Folly, see Bainton, *Erasmus of Christendom*, 91–95. Also see Lynda Gregorian Christian, "The Metamorphoses of Erasmus' Folly," *Journal of the History of Ideas*, 32 (1971): 289–94; Geraldine Thompson, *Under Pretext of Praise: Satiric Mode in Erasmus' Fiction* (Toronto, 1973), 51–85; and Walter Kaiser, *Praises of Folly: Erasmus, Rabelais, Shakespeare* (Cambridge, Mass., 1963), 91–93.

intellectual urbanity, is much closer to his actual character. The key may be in Folly's claim that shame and fear keep men from learning by experience. Shame clouds the mind, while fear advises against apparent dangers. Folly frees men from both. Few mortals realize the heights they could attain if only they could overcome these two obstacles.<sup>37</sup>

Erasmus, we suggest, was seldom free from the constraints of fear and shame. Perhaps more truly than he knew, the folly of his little *jeu d'esprit* freed him, if but briefly, from their burden. By satirizing self-importance and self-deception, Erasmus in a sense attacked his own weakness and fallibility. Paradoxically, his own folly released him from the tyranny of his shame and self-doubt, but this very release he felt compelled to parody and satirize. If these are the trappings of injured narcissism, we can infer that this burden of self-doubt not only lay at the root of his inner torment, but also served as the prod of his genius.

Once in print, the *Praise of Folly* did not go unnoticed. Erasmus believed that as a man of learning he was entitled to satirize with impunity the foibles of humanity, especially since he had provoked in the process serious reflection. Besides, the ideas and goals of the *Praise of Folly* were much the same as those of his widely acclaimed but more straightforward *Enchiridion*—advice on the true Christian life. While friends such as More appreciated his *Moriae Encomium*, others could neither forgive nor forget those large sections of the work in which they found themselves scoffed at and satirized. But it was perhaps his playful use of the text of the Holy Scripture that caused the most trouble. Many found it too adventuresome. Erasmus' attempts to convince his critics that the purpose of the work was no more than to encourage people to lead virtuous lives were not convincing. Repeatedly, he felt the need to defend it and even commented that, if he had known the book would offend so many, he might not have published it. In later years Erasmus always spoke somewhat disparagingly of this work, regarding it as a trifle, quite out of keeping with his character.<sup>38</sup>

This unique performance, the *Praise of Folly*, must be placed in the context of Erasmus' recollection of the intellectual stimulation and emotional release he had found in the company of English courtiers and humanists, especially of Thomas More. After years of demanding study in the cold confines of a Dutch monastery and amid conditions of hardship and poverty at Paris, Erasmus had encountered in England some of life's pleasures—the aristocratic diversions of banquets and hunts, the polite company of women, and what pleased him most, “men well acquainted with good learning.” Recalling these pleasures brought him joy when he was away from England. The friendly encouragement of Colet had inspired his study of Greek and the Christian classics,

<sup>37</sup> Erasmus, *Moriae Encomium* (LB), 4: 427c–28a.

<sup>38</sup> *EE*, no. 222, ll. 40–57; no. 337, ll. 26–28, 91–94, 141–45, 569–72, 585–93; no. 597, ll. 7–16; no. 622, ll. 21–30; no. 739, ll. 3–16; no. 749, ll. 3–31, esp. 7–8; no. 967, ll. 180–88; no. 1706, l. 6; no. 2463, ll. 288–309; and no. 2566, ll. 83–84.

and in the companionship of More, Erasmus confessed finding "the most delicious experience life has granted."<sup>39</sup>

More seems to have been for Erasmus the bright sun that broke through the dark clouds. In reply to Ulrich von Hutten's inquiry, Erasmus wrote in 1519 a glowing encomium of More. The portrait of More was enthusiastically idealized, possessing a quality of almost adolescent adulation. More was "the sweetest friend of all." To portray him was like trying to portray Alexander or Achilles, and even those heroes were no more deserving of immortality than More. His kindness and charm were most winning, while his wit gladdened even the most melancholic. He inspired the *Praise of Folly* and was thus responsible "for making the camel dance."<sup>40</sup> We can speculate at this point that More's charm and personal gifts stirred Erasmus' deep yearning for affection. Even though More was twenty years Erasmus' junior, he nonetheless embodied the ideals of learning and loving devotion with which Erasmus probably endowed the memory of his father. The relationship with More perhaps concealed a homosexual longing that had for long been denied—now sublimated and ennobled by an attachment to the character of the saintly Thomas More. It allowed the release and channeling of long repressed energies in Erasmus, which found expression in an outburst of playful inspiration that gave birth to the *Praise of Folly*.<sup>41</sup>

After the passage of a quarter century, Erasmus could no longer express with spontaneity his affection for More. Under Henry VIII More rose to the office of chancellor of England, only to fall from favor because his conscience would not let him take the Oath of Supremacy. Like another of Erasmus' friends, John Fisher, bishop of Rochester, More mounted the scaffold in the summer of 1535. Less than a year away from his own death, Erasmus showed the effects of years of bitter controversy. His shell had thickened, and he could no longer openly express his deep emotions. The fate of Fisher drew dry comments about England's loss and about some details of the execution; More merited both a sigh—a wish that he had left the king's business to theologians—and a philosophic confession: "In More I seem myself to have died since there was but one soul between us both, as Pythagoras says. Yet such is the turbulent course of human affairs."<sup>42</sup>

Erasmus' relation to his other major father figure, Servatius, posed a serious problem for a while. Servatius was now prior of Steyn, to which Erasmus was

<sup>39</sup> *Ibid.*, no. 103, ll. 6–25; no. 107, ll. 50–53; and no. 222, ll. 4–8.

<sup>40</sup> *Ibid.*, no. 999, ll. 19–24, 111–20. Louis Bouyer suggests that the *Moriae Encomium* should be attributed to a wholesale appropriation of and intoxication with More's style of humor; *Autour d'Érasme*, 89–90.

<sup>41</sup> One aspect of the dynamics of homosexuality involves the seeking of a narcissistic love object, someone like the idealized self. Usually this pattern is related to a deeper longing for an idealized father, for his love and approval. This may even take the form of identification with the mother, who was loved by the father. The evidence in support of this latter aspect in Erasmus is meager, but the other elements seem clear. It should be noted that More's position, gifts, and personality amply fitted him to fulfill the functions of such a narcissistic object choice by Erasmus. And Erasmus' affiliation with More provided him with a warm family context within which he was received and accepted—something his style of life had otherwise denied him. And of course, More was a loving and genial father. Consequently, the difference in age was incidental since the unconscious is little troubled by dimensions of time.

<sup>42</sup> *EE*, no. 3037, ll. 98–105; no. 3048, ll. 59–60; and no. 3049, ll. 163–64.

ultimately expected to return. Erasmus dreaded this possibility, which became more pressing when the deaths of his patron, Henry of Bergen, and his influential friend, James Batt, left him in precarious financial circumstances. A dispensation allowed him to accept benefices, however, and in a letter to Servatius dated April 1, 1506, Erasmus reported receiving the promise of a curacy from eminent and erudite Englishmen who held him in great esteem. The letter, however, was a study of repentance and renunciation, in which Erasmus demeaned himself as though he despised such worldly honors.<sup>43</sup> But Servatius knew him better than most. Yet for eight years he refrained from exercising his authority as prior to order Erasmus back to Steyn. On July 7, 1514, soon after leaving England and while a guest of Mountjoy at the castle of Hammes near Calais, Erasmus finally received the dreaded order. This terrible blow came at a time when Erasmus had the prospect of fulfilling some of his highest ambitions.

In his reply, written on the following day, Erasmus refused to return and gave a justification—an *apologia pro vita sua*. Erasmus traced the history of his unhappy vocation: how he had been pressured and shamed into entering the religious life; how he had realized that he was unsuited in mind and body for the monastic routine, but for fear of scandal had continued as a canon of St. Augustine with proper dispensation from the rules on dress; how he was revolted at the thought of returning to the boorish and ritualistic routine at Steyn; and how his health, already impaired, would give out completely there. Steyn spelled death—both mental and physical. In counterpoise to this catalogue of complaints about his unhappy vocation, Erasmus enumerated the many scholarly works he had published and influential friends he had made since leaving the monastery. On account of his literary accomplishment he was welcomed everywhere with affection and respect. He claimed as his admirers the most eminent civil and ecclesiastical leaders of England and Rome. He mentioned some of these by name, as if to list the forces he could muster to resist his return to Steyn.<sup>44</sup> The letter was a polite, if firm, rejection of Servatius' summons. It also seems to have served notice on the termination of a type of relationship.

Ironically, Erasmus addressed Servatius as his "kindest Father," and at the end of the letter said, "Farewell, once the sweetest companion of my intimacy, now a Father to be respected."<sup>45</sup> Although some of Erasmus' resentment toward the monastery at Steyn arose from the circumstances of his coming there, another factor was his relationship with Servatius, the "companion of my intimacy," who had apparently rejected his adolescent adulation and affection. And beyond Servatius there likely stood on an unconscious level the figure of his idealized, yet rejecting and abandoning, father. This rejection of his religious superior, his "Father to be respected," can be seen as a manifes-

<sup>43</sup> *Ibid.*, no. 189, ll. 1-18.

<sup>44</sup> *Ibid.*, no. 296, ll. 1-5, 8-17, 23-33, 45-69, 98-170.

<sup>45</sup> *Ibid.*, ll. 1, 236-37.

tation of the hidden and repressed anger at the abandoning father of his childhood.

Erasmus made no secret of his feelings about monastic life as practiced at Steyn and elsewhere. He described his fellow monks as lacking a spirit of Christ and priding themselves on their pharisaical ceremonial observances. He decried "the huge mass of monasteries where the practice of piety has so declined that, by comparison, brothels are more temperate and modest." The monastic obligation—"I almost said servitude—is not to be found in the Old or New Testament." And he eased his conscience by declaring that his monastic vows had been taken under duress and were, therefore, not binding.<sup>46</sup> In two letters written by his secretary, Jacopo Sadoletto, on January 26, 1517, Pope Leo X granted Erasmus dispensations that permitted him to dress as a secular priest, to hold ecclesiastical benefices despite his illegitimacy, and to live outside the monasteries of his religious order.<sup>47</sup>

Erasmus' long refusal to maintain a permanent residence reveals a deep inner restlessness. Fear of the plague, demands of health, hope of patronage, collaboration with his publishers, and reasons of scholarship and public service—all were offered as justifications for his endless pilgrimage across Europe. Even toward the end of his life, when he settled at Basel, he was still planning to move on. When contemporaries commented on his seeming instability, he replied that his home was wherever his library and furniture were located. This restlessness may have been an unconscious searching for a family life denied him in youth but enjoyed for a while in the warm and accepting households of men like Thomas More and the printer Hieronymus Froben. His anxieties lest the plague overtake him were probably rooted in the memory that it had killed both his parents. His seeming inability to become attached to a permanent residence may also have been related to a need to see himself as an abandoned victim, without home or loved ones, driven from place to place by the vagaries of cruel fate.<sup>48</sup>

ERASMUS CONTINUED TO PUBLISH WORKS which others found controversial. In the decade after leaving his teaching post at Cambridge, some of the most famous of these writings appeared in print: his edition of the Greek New

<sup>46</sup> *Ibid.*, no. 447, ll. 464–65, 554–55, 563–70. For a contrary attitude toward his vows, see *Colloquia*, "Ichtuophagia" (1526), ed. Léon-E. Halkin *et al.* (ASD), 1, pt. 3 (1972): ll. 778–80, p. 516. His unflattering comparison of monasteries to brothels shows a certain sensitivity to and defensiveness against the denunciations of clerical concubinage preached by these friars. Given the circumstances of his own birth, Erasmus on occasion was at pains to play down the wickedness of priestly concubinage and to disparage those who called attention to this evil. He favored the option of marriage for priests and monks since this would help to end concubinage, legitimize their children, and allow noncelibate priests to live in peace. See *Antibarbarorum Liber*, ed. Kazimierz Kumaniecki (ASD), 1, pt. 1 (1969): ll. 1–7, p. 75; *EE*, no. 858, ll. 417–39; no. 1188, ll. 8–14; *Epistola apologetica de interdicto esu carniū* (1522) (LB), 9: 1201a–02a; and *De conscribendis epistolis* (1521), ed. Jean-Claude Margolin (ASD), 1, pt. 2 (1971): l. 7, p. 417; l. 10, p. 418.

<sup>47</sup> *EE*, no. 517, esp. ll. 36–60; and no. 518.

<sup>48</sup> *Ibid.*, no. 296, ll. 94–101; no. 809, ll. 117–26; no. 1236, ll. 177–84; no. 1319, ll. 15–17; no. 1479, l. 181; and no. 3032, ll. 197–232. The dimensions of abandonment, deprivation, vulnerability, and victimization are the negative correspondents of the investments of the narcissistic sense of entitlement and omnipotence. See Heinz Kohut, *The Analysis of the Self* (New York, 1971).



Testament, the *Novum Instrumentum* and subsequent *Paraphrases*, his *Querela Pacis*, the *Colloquia*, and his editions of Church Fathers. These publications won him patrons and praise and a host of critics with whom he often engaged in extended public controversy. Of all his opponents, the most important was probably Martin Luther, and the issues over which they fought were neither moot points in an obscure text nor the potential for scandal in Erasmus' works. The argument was for both men essentially theological, but it also reached the core of their respective personalities and intellectual traditions.<sup>49</sup>

At the beginning of the Reformation controversy, Erasmus found himself in sympathy with Luther's call for a more interior religious experience, with his criticisms of certain Church practices and of current scholasticism, and with his return to the Scriptures. He was unhappy, however, with the harsh tone of the friar's writings, with the suspicions they stirred of Erasmus' co-authorship, and with the danger they posed of arousing further opposition to a theology based on early Christian writings.<sup>50</sup> In his initial letters to Luther and other leaders, he professed a friendly but noncommittal attitude toward the Reformer, claiming that he had not as yet read Luther's works, while advising him to avoid anger, arrogance, and ambition. He also counseled both sides to moderation and tried to arrange arbitration of the controversy.<sup>51</sup> Eventually, Erasmus realized that the disparity between himself and Luther was not merely one of tone but of teaching. Erasmus stood firmly with the humanistic reform tradition, some of whose elements he had delineated as early as 1503 in his *Enchiridion*, a tradition which called for a reform of morals and not of doctrine. By the time Luther arrived on the scene, however, the moralism of Erasmus' earlier works was already giving way to a concern for doctrine that centered on the "mystery of Christ." Luther's increasingly vehement demand for a change in Church teachings reinforced Erasmus' orientation toward traditional dogma. Even though Erasmus found himself in disagreement with aspects of the German's theology, he still shied away from drawing that ultimate conclusion that would have served as the postulate for decisive action. Only when pressured at last into confronting Luther in print did Erasmus submit to calm and careful scrutiny the Wittenberg theologian's position on free will.<sup>52</sup> Because Luther responded with open invective to the Dutchman's gentle, but semi-Pelagian, treatise, Erasmus felt himself

<sup>49</sup> For a clear presentation of the differences between their respective intellectual traditions, see O'Malley, "Erasmus and Luther," 47-64. For a psychoanalytic study of the roots in Luther's personality for his embittered attacks on the Church, see Erikson, *Young Man Luther*.

<sup>50</sup> For a brief summary of the principal features of Erasmus' reform program, see Cornelis Augustijn, *Erasmus en de Reformatie: Een onderzoek naar de houding die Erasmus ten opzichte van de Reformatie heeft aangenomen* (Amsterdam, 1962), 305. *EE*, no. 939, ll. 44-113; no. 980, ll. 4-10; no. 1033, ll. 59-175, 195-200, 204-28, 238-40; no. 1119, ll. 36-37; no. 1141, ll. 24-30; no. 1153, ll. 30-38; no. 1156, ll. 35-48; no. 1202, ll. 31-50, 207-12, 257-60; and no. 1688, ll. 28-35.

<sup>51</sup> *EE*, no. 980, ll. 17-18, 37-51; no. 1033, ll. 38-40, 50-58; no. 1141, ll. 8-13; and no. 1202, ll. 34-65, 178-84, 262-66.

<sup>52</sup> Erasmus, *De libero arbitrio diatribe sive collatio* (LB), 9: 1215-48, and his later reflections in *De amabili ecclesiae concordia* (LB), 5: 500b-c; and Georges Chantraine, "Mystère" et "Philosophie du Christ" selon Érasme: Étude de la lettre à P. Volz et de la "Ratio verae theologiae" (1518), Bibliothèque de la Faculté de Philosophie et Lettres de Namur, part 49 (Gembloux, Belgium, 1971), 227-35, 368-93.

wronged. His initial attempts at arbitration and subsequent dissociation from the controversy had proved unsuccessful. Pressured into taking a public stand, he sided with the traditional Church. While he affirmed his own orthodoxy, his sympathies on many nondogmatic points lay with the reformers, although for reasons different from theirs.<sup>53</sup>

Erasmus' stance in the Lutheran controversy can be variously explained. He was part of—indeed, an unwilling spokesman for—that group known as the Moderates, which included among its adherents important men from the liberal wings of both the traditional Church and the camp of the reformers. The dynamics of polarization which exaggerated the distance between Wittenberg and Rome made this moderate position increasingly difficult for Erasmus to maintain. He was under both external and internal pressures to side with one of the groups against the other. Catholic conservatives like Pio and Aleander questioned his orthodoxy when he refused to join their party. Followers of Luther accused him of betraying the Gospel out of cowardice and in hopes of personal profit. Forces within his psyche also urged him to abandon the religious position he had evolved over many years. To side with the more destructive elements within the Lutheran faction would have allowed him to vent his resentments against the parental Church, but would also have meant giving up the security and authority it represented. To join the conservative camp within the Church would have implied a rejection of the rebellious spirit Luther embodied and an endorsement of the Church with all its faults. Erasmus could disown neither his resentments nor his longings for acceptance. By maintaining this moderate religious stance, he could not only remain consistent with his long-standing religious beliefs, but also leave unresolved this transferred ambivalence toward the parental Church. He was both drawn to controversy and repelled by it.<sup>54</sup>

Erasmus was probably sincere in professing a dislike for confrontation, but polemics provided him with an outlet for his pent-up resentment. Once engaged in verbal combat, he could not resist inflicting wounds with his derisive and rapierlike pen. The venom of his attacks and the apparent relish

<sup>53</sup> *EE*, no. 1688, ll. 2–9; no. 1033, ll. 256–60; and Erasmus, *De amabili ecclesiae concordia* (LB), 5: 497c–98b. Augustijn has traced the humanist's stance with regard to the reformers in much greater detail: *Erasmus en de Reformatie*, esp. pp. 305–09. The publication of Luther's *De captivitate Babylonica* in 1520 apparently hindered Erasmus' first attempts at securing an arbitration, but he tried again under Adrian VI and Clement VII only to find both sides intransigent. For several years Erasmus thought the controversy was over nonessentials, but he became aware of significant differences when the debate focused on the Eucharist. Then Erasmus came to a recognition of the doctrinal issues at stake and to a deeper appreciation for the role of tradition in interpreting the Sacred Scriptures. He felt that the failure of the Diet of Augsburg effectively ended all hopes of compromise. During the last fourteen years of his life Erasmus more and more came to accept the traditional Church and urge that reform remain within its fold.

<sup>54</sup> Smith, *Erasmus*, 440; and Phillips, *Erasmus and the Northern Renaissance*, 219–21. Erasmus' posture in contrast with Luther's more declarative stance was echoed in a well-known sixteenth-century epigram: "Where Erasmus merely nodded, Luther rushed in; where Erasmus laid the eggs, Luther hatched the chicks; where Erasmus merely doubted, Luther laid down the law"; as quoted in Crane Brinton *et al.*, *A History of Civilization*, 1 (Englewood Cliffs, N.J., 1957): 453. For a detailed study of his relations with one of his conservative Catholic opponents, see Myron Piper Gilmore, "Erasmus and Alberto Pio, Prince of Carpi," in Theodore K. Rabb and Jerrold E. Seigel, eds., *Action and Conflict in Early Modern Europe: Essays in Memory of E. H. Harbison* (Princeton, N.J., 1969), 299–318.

with which he launched them are perhaps most clearly seen in his comments on monks and theologians. While Erasmus sincerely wished to live in peace and quiet and to dedicate himself to "good learning," he also needed to justify himself in the eyes of the world, as well as in his own eyes. Toward the end, the constant struggle took its toll. He confessed to being so exhausted that he was seriously thinking of withdrawing from controversies.<sup>55</sup> In the closing decade of his life, Erasmus retired from the Reformation battles at Basel for the quiet of Freiburg im Breisgau, continued his critical editions of the Fathers, and penned works of piety and pleas for peace: on preaching—*Ecclesiastes sive De Ratione Concionandi* (1535), on mending the peace of the Church—*De Sarcienda Ecclesiae Concordia* (1533), and on preparing for death—*De Preparatione ad Mortem* (1534). He returned to Basel in 1535, dying there on July 12, 1536.

A REMARKABLE DISPARITY EXISTED BETWEEN the public persona and intellectual achievement of Erasmus and his personal attitudes and behavior. Although hailed as the leading humanist of his day and esteemed as a gentle man of peace and moderation, he was often haunted by unhappiness, dissatisfaction, and despondency. In his dealings with others he usually maintained his distance, was guarded and suspicious, and at times even spitefully small and resentful. Self-doubt and insecurity pervaded his life. Although philosophic considerations and social conditions not unlike those common among Italian humanists may have reinforced tendencies already within his character, they do not of themselves adequately explain his pessimism.<sup>56</sup> Poor health, faithless friends, inveterate poverty, and Herculean labors without reward pursued him relentlessly—or at least so it seemed.<sup>57</sup> He presented himself as utterly wretched and worthy of mankind's scorn, riddled with self-contempt and self-doubt. Revenge on his enemies—his weapon was the pen—could on occasion give him reason to go on living.<sup>58</sup> Even marriage promised no happiness—"Any man who marries must prepare his mind for the

<sup>55</sup> *EE*, no. 237, ll. 6-7; no. 476, ll. 67-71; no. 980, ll. 46-51; no. 2136, ll. 150-84; and no. 2522, ll. 53-65.

<sup>56</sup> Charles Edward Trinkaus, *Adversity's Noblemen: The Italian Humanists on Happiness* (New York, 1940), 141-50.

<sup>57</sup> *EE*, 1: 51, *Compendium Vitae*, ll. 141-44; no. 31, ll. 5-11; no. 83, ll. 85-102; no. 145, ll. 51-62; no. 296, ll. 23-33; no. 325, ll. 58-63; no. 551, ll. 3-4, 15-16; no. 552, ll. 10-11; no. 893, ll. 1-28; no. 1102, ll. 2-9; no. 1136, ll. 19-20; and no. 1352, ll. 115-17. For the plottings of Eppendorff against him, see no. 1437, ll. 8-209. For a recent treatment of Erasmus' physical ailments which raises the question of psychological origins, see Hyacinthe Brabant, *Érasme: Humaniste dolent* (Quebec, 1971), 103-04.

<sup>58</sup> Erasmus on occasion could be so depressed that he almost longed for death as an end to his woes. See, for example, *EE*, no. 83, ll. 90-109; and no. 2798, ll. 26-37. On one occasion, when he thought his death was imminent, he was determined to strain every nerve in order to get a fitting revenge on his critics before the end; see *EE*, no. 138, ll. 44-57. The desire for revenge remained with him even when fame and fortune came his way. Granted that bitter polemics are readily found in the words of some of the humanists, the biting passages in Erasmus' writings, while generally much muted in comparison, are not mere imitations of his peers' invectives nor only a response in kind but replies that served his own inner needs.

eventualities that are common to mankind: sterility in his wife, childlessness, loneliness.<sup>59</sup>

Erasmus was disillusioned. The religio-humanistic ideals of peace and concord, which, together with moderation, he so fervently espoused, were seldom realized.<sup>60</sup> A smoldering rage seemed to color his experience and to diffuse itself more generally against all mankind. It was a rage he recognized and tried to rationalize: "Besides, one whose criticism leaves no class of men untouched is, it would seem, angry at no single man but at all forms of vice. Therefore, if there is anyone who will cry that he has been insulted, he will betray either a guilty or certainly a worried conscience." Buried in this passage is one of those clues for which the psychologist looks. Erasmus may well have been angry at *one single man*—the father who had rejected and abandoned him. This anger was, however, displaced as generalized hostility.<sup>61</sup> Erasmus saw himself abandoned in a world where there were few wise men (one of whom was himself) and many who were willing to settle for those illusions that sustained their shallow happiness. Not only did the world ignore his wisdom, but his literary works and the taxing and time-consuming task of editing old manuscripts seemed to win him little recognition or appreciation and brought upon him only a storm of ill will. He was dissatisfied with himself and his work. Almost as soon as his publications came off the press he found it necessary to revise and supplement them. The burdens of scholarship weighed upon him, and he longed to escape. In his brief autobiography he appears discontented with his literary accomplishments. While he held wealth and honor in contempt, he treasured leisure and freedom for scholarly pursuits.<sup>62</sup>

From the mouth of Folly came a description of the "wise man," which must pass at least in part as a self-portrait:

Imagine some model of wisdom to set up against this fool, a man who has frittered away all his boyhood and youth in acquiring learning, has squandered the most pleasant part of his life in endless vigils, cares, and labors. Lest indeed in the whole remainder of his life he taste even a drop of pleasure, he is always thrifty, impoverished, sad, gloomy, harsh and unjust to himself, severe and unpopular with others, worn out with a pallor, thinness, ill-health, sore eyes, and with an old age and hoary

<sup>59</sup> *Ibid.*, no. 541, ll. 1–6; no. 2684, ll. 35–39; he could also express conditional optimism on the chances for a happy and fruitful marriage, e.g., *Colloquia*, "Proci et Puellae" (1523) (ASD), 1, pt. 3: ll. 227–357, pp. 283–87.

<sup>60</sup> We do not want to give the impression that Erasmus was always disillusioned with life or disappointed in his friends. He experienced bursts of optimism, and he also maintained a number of close friends even in his periods of deepest despondency. These positive experiences, however, did not significantly alter his more typical and habitual patterns of thinking, feeling, and relating to others.

<sup>61</sup> *EE*, no. 222, ll. 62–68. The psychological interpretation of this passage requires some explanation. On a conscious level, Erasmus was protesting equivalently: "If my criticism touches all classes of men, then I am not angry with any one man but with all vices." An unconscious translation would take the following form: "If I criticize all men, it is because I am really angry at one." The one primary figure who stood forth as the leading object of this denied, repressed, and unconscious resentment was Erasmus' abandoning father. In the clinical setting the analyst might wait for additional evidence to confirm this hypothesis. The psychohistorian can only appeal to its congruence with other known elements of the subject's life. For the role of negation as the conscious substitution for repression, see Sigmund Freud, "Negation" (1925), in *Standard Edition*, 19 (1961): 233–39.

<sup>62</sup> Erasmus, *Moriae Encomium* (LB), 4: 431d–452a; *EE*, 1: 51, *Compendium*, ll. 141–45; 2: 183–84; no. 325, ll. 58–63; no. 402, ll. 1–3; no. 966, ll. 30–33; and no. 2299, ll. 103–08.

crop of hair acquired long before their time, someone fleeing from life before his final day. And yet what difference does it make when a man like that, who has never lived, does die? There you have that admirable image of a wise man.<sup>63</sup>

None of this, of course, rings entirely true; the image which Erasmus presents of a humble, self-effacing, and deprived scholar is only half of the picture. Even his attempts at resignation, honest and sincere though they may have been, are not really convincing. Erasmus pictured himself as resignedly playing the part assigned to him in life, yet he continued to grumble, for he saw his role as a lowly and miserable one.<sup>64</sup> On a deeper level existed another Erasmus who was arrogant and vengeful, intent on his own convenience, and resentful of his lot in life. He found this Erasmus difficult to acknowledge either to himself or to others.

Although he continued to long for intimacy, he kept his friends at a distance, as though he feared they might recognize his vulnerability, which may have been related to, but not entirely derived from, the circumstances of his birth. The threat posed by intimacy made him suspicious of friends and fearful of ever being his idealized self—the open, sincere, and loyal friend with only his heart to offer. His difficulties in establishing close friendships had a deeper origin in his narcissism. He was fundamentally a solitary person, whose loneliness was in large measure self-centered. Friends, he found, could be fickle and inconstant—and he turned from them to learning, the only thing that seemed to survive the flux of fortune. His ideas, his accomplishments, his position, his name increasingly became the criteria for viewing everything else. The fame which scholarship won only served to reinforce that side of him which saw the world in terms of his own needs and ambitions. As the years passed, his enormous correspondence became little more than a protracted self-justification and self-defense.<sup>65</sup>

Particularly galling to the independent spirit of Erasmus was his need for financial support, particularly in the early years of his career. The necessity for begging and wringing money from his friends, however willing they were to contribute it, was a source of inner discontent.<sup>66</sup> Early on, Erasmus sought to make himself less dependent on unreliable patrons by taking back to the Continent the money he accumulated while in England during 1499–1500. Having been misinformed by More and Mountjoy that a recent law prohibiting the export of species did not apply to foreign currencies, he watched helplessly while English customs officials confiscated his savings as he was about to sail. The English tyrant had robbed him of the rewards of his learning, yet Erasmus feared to lose the affection of Englishmen like Mountjoy should he take revenge with his pen. Instead, he published a collection of classical proverbs known as the *Adagia*, dedicating it to Mountjoy. But his

<sup>63</sup> Erasmus, *Moriae Encomium* (LB), 4: 438e–39a.

<sup>64</sup> *EE*, no. 2443, ll. 417–19.

<sup>65</sup> *Ibid.*, no. 107, ll. 44–46; no. 272, ll. 1–5; no. 1206, ll. 113–14; and no. 145, l. 70; and Huizinga, *Erasmus*, 122.

<sup>66</sup> *EE*, no. 138, ll. 36–44; no. 139, ll. 25–31, 50–52; no. 145, ll. 51–99.

bitterness lingered. Nine years later, Mountjoy hoped that the accession of Henry VIII would finally end his resentment.<sup>67</sup> The episode is revealing. Fate had snatched a comfortable sum of money from his pocket; to be without money was to become like a slave.<sup>68</sup> The misfortune at Dover demonstrates Erasmus' readiness to see himself as oppressed, to cast blame even on a helpful friend, and to remain for years resentful of an injury.

The marked ambiguity of Erasmus' mind can be interpreted as a matter of philosophic conviction. But it also reflects his basic indecision, ambivalence, and reluctance to commit himself to a decisive course of thought or action. He asserted, while behind the mask of Folly, that "man's happiness . . . depends on opinions. For human affairs are so varied and obscure that nothing can be clearly known of them, as has been rightly stated by my Academicians, the least arrogant of the philosophers. . . . Finally, man's mind is so formed that it is far more susceptible to pretence than to truth."<sup>69</sup> In his debate with Luther, Erasmus expressed this same sympathy with the academicians or skeptics, but was careful to circumscribe it: "And, in fact, so far am I from delighting in 'assertions' that I would readily take refuge in the position of the Skeptics, wherever this is allowed by the inviolable authority of the Holy Scriptures and by the decrees of the Church, to which I everywhere willingly yield my assent, whether I grasp what it prescribes or not."<sup>70</sup> When revealing his inner convictions, Erasmus often took pains to qualify or disavow them. This is perhaps most evident at the end of the *Moriae Encomium* where he disclaimed his many critical statements on the artful ground that they came from the mouth of Folly. Confrontation was not to his taste. There was frequently a reserve, an uncertainty, a partial displacement of the center of intention, so that his readers were never sure whose sentiments and opinions they were reading. He tended to avoid direct action, not merely out of suspicion or caution, but because he immersed himself in the eternal uncertainties of human existence.<sup>71</sup>

This desire to persevere in ambiguities is evident in his relationship with Martin Luther. When Luther first openly sought his support, Erasmus retreated to a policy of studied ambiguity. This action was consistent with his fear of controversy, his inner sense of weakness and vulnerability, and also his characteristic dislike of taking sides with any person or any cause save those

<sup>67</sup> *Ibid.*, 1: 16–17, no. 1, ll. 1–10, 19–37; no. 145, ll. 53–58, 92–94; and no. 215, ll. 23–27.

<sup>68</sup> *Ibid.*, no. 119, ll. 7–8; no. 139, ll. 51–52; and no. 227, ll. 20–22.

<sup>69</sup> Erasmus, *Moriae Encomium* (LB), 4: 450c; and *EE*, no. 713, l. 18.

<sup>70</sup> Erasmus, *De libero arbitrio diatribe* (LB), 9: 1215d.

<sup>71</sup> *EE*, 5: 76, no. 1292. Allen's introduction quotes B. Hubmaier on Erasmus' reluctance to write down his opinions on controversial matters; while commenting on controversial topics in his *Colloquia*, Erasmus claimed a different purpose. See *EE*, no. 1262, ll. 11–15; and no. 1476, ll. 8–11; and Preserved Smith, *A Key to the Colloquies of Erasmus*, Harvard Theological Studies, no. 13 (Cambridge, Mass., 1927), 2. In his *De utilitate colloquiorum* (1526/29) (ASD), 1, pt. 3 (1927): ll. 29–368, pp. 742–51, Erasmus admitted that his purpose went beyond teaching a refinement of language; *EE*, no. 337, ll. 89–120, 432–48, 542–47; *The Polemics of Erasmus of Rotterdam and Ulrich von Hutten*, trans. and annotated by Randolph J. Klawiter (Notre Dame, 1977), p. 209, section 243; and Erasmus, *Moriae Encomium* (LB), 4: 504c; and *Responsio ad Annotationes Eduardi Lei in Erasmus Novas* (1520) (LB), 9: 273b. Erikson refers to him as "Erasmus the All-Adjustable"; *Young Man Luther*, 147.



most general causes of religious reform and "good learning." In addition, it conformed to his deep and abiding conviction that neither of the conflicting opinions could express or encompass the whole truth and that human passions of hatred and excessive enthusiasm tend to distort men's minds. While committing the final outcome of the conflict to Christ, he eventually embraced an active policy of mediation. "I urge everyone I can to keep from taking sides and exhort both factions to come together on fair terms, if possible, so that little by little harmony might return."<sup>72</sup> This position was also characteristically Erasmian. He opted for the safe middle course, preserving all aspects of ambiguity, committing himself to no faction, and holding what proved to be a vain hope that, by not giving offense, peaceful accord would be achieved.

Erasmus' style was neither to seek out followers nor to form a faction. He avoided leadership, even of the so-called Moderates. He claimed no disciples for himself, but urged all to follow the teachings of Christ and persevere in the traditions of the Church.<sup>73</sup> If his loyalties remained with the Church, he refused to commit his prestige and energies to an ardent defense. The *De Libero Arbitrio Diatribe* was written reluctantly, and the leadership it offered was transient and ephemeral. His craving for affection and acceptance led him to try to give the least displeasure to the most people and, if possible, to placate all. Ironically, this effort aroused the wrath of both parties. His real enemies, he claimed, were only those who opposed good learning and evangelical truth.<sup>74</sup>

IN APPARENT CONTRADICTION TO HIS LONGING for acceptance was Erasmus' desire to play the part of a silent victim, one who avoided controversy wherever possible and did not respond to personal attacks.<sup>75</sup> Beneath this "Erasmian" spirit of moderation lay the wish to be a patiently suffering, disadvantaged, and unfortunate victim. Although Erasmus' personality contained many vagaries that can be regarded as more or less normal, he also displayed paranoid tendencies. He was plagued by persecutors: boorish teachers who sought to break his spirit, guardians who wasted his inheritance and pressured him into the monastery, a brother who left him in the lurch, false companions who seduced him into the religious life, fellow monks who opposed his literary interests, professors at the Sorbonne who squandered his time with their "supersubtleties," patrons who let him live in poverty, customs officials who robbed him of his earnings, ignorant critics of his *Moriae Encomium* and *Novum Instrumentum* who ridiculed him before the illiterate rabble, Luther and his followers who accused him of skepticism and Epicureanism, conservative Catholics who blamed him for having laid the egg

<sup>72</sup> *EE*, no. 337, ll. 414-29; no. 933, Introduction and ll. 1-37; no. 939, ll. 87-102; no. 980, esp. ll. 17-18, 47-49; no. 1033, ll. 57-68; and no. 1526, ll. 140-44.

<sup>73</sup> *Ibid.*, no. 2690, ll. 37-40; and no. 2845, ll. 3-6, 33-38.

<sup>74</sup> *Ibid.*, no. 1156, ll. 35-45; no. 1167, ll. 436-41; no. 1352, ll. 42-78, 115-17; no. 1411, ll. 13-28; no. 1419, ll. 2-3; no. 1488, ll. 24-35; no. 1496, ll. 167-92; no. 1526, ll. 48-85; and no. 1670, ll. 20-24.

<sup>75</sup> *Ibid.*, no. 2443, ll. 115-19.

which Luther hatched, Heinrich von Eppendorff whose spies constantly surveyed his activities and even intercepted his correspondence, Aleander who set others in opposition to his ideas and wishes and stood guard lest Erasmus withdraw from the Catholic camp, and Italians who slandered him for mocking their national pride. The list seems endless.<sup>76</sup>

Erasmus saw himself as an innocent victim struggling for survival. After spending himself in the service of others by his dedication to belles-lettres and to the advancement of true religion, he seemed to have gained little but hostility. The right of self-defense was the least he could claim. Even his favorite saint, the irascible Jerome, had retaliated on less provocation.<sup>77</sup> Erasmus dealt with attackers in a number of ways. While avoiding the coarse invectives typical of his time, he did resort to occasional name-calling and dismissed some critics as ignorant slanderers, playing to ruffraff. He accused others of envy and fear of his learning. He tried to ignore the injuries inflicted on him but was not always successful.<sup>78</sup> Although Erasmus longed at times for vengeance, he was ambivalent about it. The need to refute false charges of impiety, he insisted, was all that led him to counterattack. The desire for revenge he discounted as a pinprick that touched the surface and went no deeper. His advice as a Christian was to ignore provocation, provided one's silence did not lead to scandal.<sup>79</sup> In spite of such denials and restraints on venting anger, Erasmus repeatedly succumbed to his desire for revenge. His attack on the Scottish guardian of his former pupil Thomas Grey was spiteful and scurrilous. In his quarrel with the youthful critic Edward Lee, Erasmus impugned his opponent's motives and belittled his abilities. Ulrich von Hutten did not live long enough to read Erasmus' vindictive *Spongia Adversus Aspergines Hutteni* (1523). Hutten's defender, Heinrich von Eppendorff, was in turn mercilessly defamed.<sup>80</sup> As in other incidents, a false charge of impiety was not Erasmus' sole reason for retaliation.<sup>81</sup>

To rationalize and justify his paranoid disposition was also consistent with Erasmus' personality. He spiritualized it so as to see himself as a second Suffering Servant, a humble sinner, the innocent victim of evil and malicious

<sup>76</sup> *Ibid.*, no. 1268, ll. 61-75; no. 1342, ll. 134-47; no. 1352, ll. 91-104; no. 1479, ll. 184-85; no. 1528, ll. 11-17; no. 1690, ll. 18-47; no. 1934, ll. 255-356; no. 2395, ll. 2-4; no. 2400, ll. 27-36; no. 2565, ll. 7-13; no. 2892, ll. 37-44; no. 2906, ll. 67-70; no. 2936, ll. 46-77; no. 3032, ll. 115-601, esp. 233-70; no. 3130, ll. 17-18; and 11: 226, n. to no. 3052, together with 11: 322, Introduction to no. 3120. These last two references suggest that Erasmus' paranoid disposition had reached the frankly delusional state of a "diseased imagination."

<sup>77</sup> *Ibid.*, no. 222, ll. 68-72; no. 778, ll. 222-50; no. 1139, ll. 67-75; no. 1144, ll. 15-19; and no. 1352, ll. 115-17.

<sup>78</sup> *Ibid.*, no. 64, ll. 89-92; no. 108, ll. 20-37; no. 138, ll. 53-58; no. 337, ll. 327-29; no. 476, ll. 67-69; no. 541, ll. 81-82; no. 809, ll. 18-21, 64-67, 83-84; no. 2443, ll. 115-19; and no. 2651, ll. 38-42.

<sup>79</sup> *Ibid.*, no. 778, ll. 237-38; no. 980, ll. 46-49; no. 2136, ll. 150-84; and no. 2443, ll. 83-84.

<sup>80</sup> *Ibid.*, no. 58, ll. 13-126; no. 998, ll. 26-55; no. 1053, ll. 197-207, 299-317; no. 1437, ll. 8-9; no. 1934, ll. 251-435; no. 2088, ll. 125-30; and *The Polemics of Erasmus and von Hutten*, 177, 186, 196, 208, 221, 223, 233, 240-41, 243, secs. 127, 163, 197, 236, 294, 304-05, 354, 390, 393, 399, 402.

<sup>81</sup> *EE*, no. 138, ll. 49-51; no. 2203, ll. 8-12; and no. 2443, ll. 46-196. The motif of revenge is closely interwoven with the fabric of narcissistic vulnerability, sensitivity, and injury. See Heinz Kohut, "Thoughts on Narcissism and Narcissistic Rage," *Psychoanalytic Study of the Child*, 27 (1972): 360-400. Kohut's description of aspects of narcissistic rage is a general one and not directly applicable in every way to Erasmus. Its purpose is to support the links between the revenge motif and other aspects of a narcissistically vulnerable character structure.

men, who abused him and even sought to drive him to the cross. Behind their actions Erasmus saw the hand of God scourging him to correct him. He was willing to submit to God's judgments with patience and peace.<sup>82</sup> How better to be a suffering victim than to be scourged by the hand of God?

The whole corpus of Erasmian writings was, in a sense, an elaborate self-justification. When Erasmus described himself to Colet, he combined, while protesting the contrary, false modesty and boasting: he was poor in material goods since he shunned ambition; he lacked learning but admired it ardently; he acknowledged his own deficiencies, while esteeming the good in others; he was unassuming, for he decried pretense; in sum, he was diffident but honest. As if to balance off these few negative qualities, he concluded by claiming to be "simple, open, free, with . . . only his heart to offer"<sup>83</sup>—a rather idealized self-conception! Now and again appeared a touch of self-glorification, even grandiosity. With rhetorical flourish he wrote to his friend Batt, comparing himself to other theologians: "Their sermons are full of trite sayings; I am writing words destined to live forever. Their ignorant nonsense is heard in one church or other; my books will be read by students of Latin and Greek in every nation of the world. A superabundance of ignorant theologians exists everywhere, while one like me is rarely found in many centuries." But then he felt compelled to disqualify what he had written as "a few little white lies."<sup>84</sup>

Side by side with these dark elements in Erasmus' character were ennobling qualities. He embraced honorable ideals and longed for a Christian society raised on an ethical basis and permeated with a fervent faith. He passionately desired simplicity, gentleness, kindness, moderation, tolerance, and peace. For him the road to true happiness lay along the path of "good learning" and particularly the study of Sacred Scripture and of ancient commentaries upon it. He affirmed that a truly religious impulse lies in the direction of charity and love of neighbor, which should manifest itself in doing good for others no matter how meager the rewards.<sup>85</sup> Yet, to a significant degree, the ideals and virtues that he espoused were not altogether divorced from the more pathological aspects of his character. Thus, for example, his ardent advocacy of gentleness, moderation, tolerance, and peace arose in part from his need for a defense against his sense of vulnerability. And yet he himself was repeatedly guilty of the very anger, arrogance, and pettiness he urged others to avoid.

The contradictory and enigmatic element in the character of Erasmus is revealed by the dispute over his motto, *Concedo nulli* ("I yield to no one"). His critics charged that it showed an arrogance that Erasmus vehemently denied. His spirit, he protested, was closer to the Socratic adage, "This alone I know, that I know nothing."<sup>86</sup> But his denial and labored rationalization ring

<sup>82</sup> *Ibid.*, no. 2892, ll. 132-43; and no. 2205, ll. 23-58.

<sup>83</sup> *Ibid.*, no. 107, ll. 38-46.

<sup>84</sup> *Ibid.*, no. 138, ll. 63-66, 82-85; no. 139, ll. 36-42, 60-62; see also no. 421, ll. 100-02; and no. 2299, ll. 103-13. With due regard to the rhetorical tradition evident in letters such as no. 139, we maintain that psychological factors are also operative.

<sup>85</sup> *Ibid.*, no. 186, ll. 10-12; no. 237, ll. 83-85; and no. 364, ll. 26-32, 38-42.

<sup>86</sup> *Ibid.*, no. 2018, ll. 1-72.

hollow. Curiously, he argued that the motto did not apply to himself, a position similar to one already noted in his *Praise of Folly*. Erasmus' defense suggests that the charge contains the seed of truth. Erasmus was, indeed, an arrogant man. His urbane and gentle bearing and the polished style of his writings were an elaborate attempt to veil this inner arrogance—itsself the mask of his deeper sense of vulnerability and victimization.

ENOUGH FRAGMENTS HAVE BEEN GATHERED to assemble a final portrait. In so doing it is important to note that the perspective and language change. Until now emphasis has been on documenting aspects of Erasmus' character and situating them in the context of his life and times. Attention to the limitations of more commonly accepted historical evidence has resulted in those numerous qualifications on statements about his character that probably appear overly cautious to the experienced psychoanalyst. He is also not satisfied with the aggregate data, no matter how valid, until it begins to take shape in some consistent and clinically recognizable form. In conclusion, then, emphasis on a cautious historical investigation and validation will be replaced by concern for a clear and coherent psychoanalytic interpretation of Erasmus' character.

The thread that joins many aspects of his character is injured narcissism. From the very beginning his self-esteem was under attack. His early life, as described in his letters and in the *Compendium Vitae*, was for him a continual saga of deprivation, disappointment, frustration, resentment, and caustic criticism of many with whom he came in contact—a bitter tale of entitlements denied and expectations disappointed. The absent and longed-for father cannot be taken as the single cause for the injury. Nor can it conclusively be asserted that the circumstances of Erasmus' birth, his illegitimacy, and the early abandonment by his father did, in fact, shape his adult character. But it can be said that the overall *Gestalt* of Erasmus' personality and behavior is congruent with such a model derived from clinical experience. Undoubtedly, other hypotheses could be generated to explain the data, but such hypotheses, too, would have to meet the same criteria of validity and are likely to be less convincing than the more obvious one on which this article is based.

Erasmus lived under what Gregory Rochlin calls "the tyranny of narcissism."<sup>87</sup> His life appears to have been an incessant seeking for recognition, acceptance, and adulation from his fellow man. Questioning or criticism of his

<sup>87</sup> Rochlin, *Man's Aggression: The Defense of the Self* (Boston, 1973), esp. 217–48. Since narcissism is central to an understanding of the character of Erasmus, some definition of the term may be useful. Rochlin's description will serve this purpose: "The compelling imperative for self-preservation is self-love. It expresses itself in an endless lust for a rewarding image of oneself, whether that image is seen in a glass or in another's eye. The further passion for praise, honor and glory makes for an endless marathon. We enter it remarkably early in our existence and leave it only when we expire. Self-love is a process subject to development, responsive to the vagaries of our fantasies as well as to the circumstances of our lives; it is a governing tyrannical principle of human experience, to which aggression responds as a bonded servant. Neither metaphor nor a mere label, narcissism, this love of self, is the human psychological process through which preserving the self is assured. In infancy, childhood, maturity and old age, the necessity of protecting the self may require all our capabilities. And, when narcissism is threatened, we are humiliated, *our self-esteem is injured, and aggression appears*"; Rochlin, *Man's Aggression*, 1.

motives or performance could prompt in him an anxious and threatened response which mobilized his resources and rushed them to the defense of his embattled narcissism. Erasmus' sense of self-worth and self-esteem was continually in jeopardy and required constant reinforcement. His feelings of inadequacy were reflected in his depression, disillusionment, skepticism, and isolation. Much of his Herculean literary labor can be seen as a gigantic effort to redeem some sense of inner value, as though he sought to build an outer facade of accomplishment and sophistication to conceal a self-perception of worthlessness and vileness.

Erasmus' despondency, the result of denied or frustrated narcissism, can be related to the paranoid dimensions of his character. His inevitable inability to achieve his narcissistically invested high standards—whether of piety, peace, friendliness, moral reform, scholarly industry and expertise, or of some other ideal—was experienced by him as a shameful and intolerable failure, bringing in its wake an increased sense of inadequacy, worthlessness, and shame. To protect himself against these painful depressive effects, he resorted to the narcissistic defense of paranoid projection and denial. He read into the faces of others the self-contempt he more than likely felt due to his own failures. To compensate for this injured self-esteem, he also wanted to devalue and scorn others, while secretly envying their apparent well-being. He looked on them as opponents, projected onto them his own desires to demean an adversary, and expected that these hostile feelings would be returned. Although any scholar in his position could not avoid a certain amount of criticism and opposition, Erasmus was led on occasion, because of these paranoid projections, to verify his expectation of enemies by eliciting hostile responses and by depicting himself as their helpless victim. He also resorted to the paranoid defense of denial, absolving himself of responsibility for his failings, and casting blame on others whom he placed in the role of his persecutors, whether they were his Scotist teachers, religious superiors, or fellow scholars. His guardedness, suspiciousness, inability at times to trust or become firmly attached to his friends, repeated persecutory preoccupations, and even frankly delusional states of paranoia toward the end of his life also reflect this aspect of his personality. The paranoid attitudes and behavior of Erasmus can thus be placed within a clinical framework which sees connections between this neurosis, a defense against despondency, shame at failing to achieve lofty ideals, compensations for feelings of inadequacy, and the painful experience of being illegitimate and abandoned.<sup>88</sup>

On the basis of clinical experience, a number of hypotheses can also be made that relate certain aspects of Erasmus' career development to what was reported about his earliest years. Through his dedication to classical letters and to religion, he unconsciously attempted to identify with his learned priest-father and to win the approval of his pious Latin teachers, the father figures

<sup>88</sup> For a fuller treatment of the inner logic of narcissism and paranoid distortions, see J. M. Murray, "Narcissism and the Ego Ideal," *Journal of the American Psychoanalytic Association*, 12 (1964): 477-511.

most available to him. His numerous literary and scholarly writings represented an endless endeavor to win from a host of paternal substitutes the acceptance and approval denied him by his abandoning father. Erasmus' sweeping satirical attacks, especially on such father figures as prelates and teachers, allowed him to vent the repressed anger he felt for his unavailable father. The ambivalent relations Erasmus maintained with the Church were the product in part of an unconscious transference of his conflicting feelings about his father. To reject the Church would have implied surrendering his need to both attack and cling to it, or in psychological terms, both to rage against and to seek his abandoning father. To defend it would have meant depriving himself of the object of his wrath and admitting that the good element in the Church far outweighed those deficiencies the criticism of which was so important to him. By siding with neither conservative Catholics nor Protestant reformers, Erasmus found himself attacked by both and could thus preserve his sense of victimization. The course of Erasmus' career can, therefore, be seen as conforming in many ways to the psychological needs which clinical experience suggests could have arisen from the peculiar circumstances of his birth and early childhood. If injured narcissism drove Erasmus to extraordinary efforts to fulfill the demands of paternal identification and gain acceptance and approval through learning, that same narcissism would have set the limits and inhibitions that significantly contributed to the course of action he followed in his historical context.



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## Ritualistic Acts and Compulsive Behavior: The Pattern of Tudor Martyrdom

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SEYMOUR BYMAN

SHORTLY BEFORE HIS DEATH IN 1555, the Marian martyr Laurence Saunders condemned the highly ceremonial nature of the Roman Catholic religion. "Even from the beginning," he declared, ceremonies had been "invented and ordained for the rude infancy and weak infirmity of man." That the primitive church had few ceremonies was proof of its greater "perfection." By contrast, the many ceremonies of the late "church papistical"—"partly blasphemous, partly unsavory, and unprofitable"<sup>1</sup>—were evidence of its "rudeness." Yet Saunders himself displayed a ritualistic pattern of behavior replete with ceremonial devices through much of his life. "In study he was diligent and painful; in godly life he declared the fruits of a well exercised conscience; he prayed often and with great fervor; and in his prayers, as also at other times, he had his part of the spiritual exercises. . . ."<sup>2</sup> Saunders was not alone. Many Marian martyrs prepared themselves for death in ways reminiscent of Catholic ceremony. They devised a compulsive ritual that they performed meticulously in exacting detail. This elaborate ceremony followed an inflexible pattern: a precise, self-disciplined daily existence, culminating in a ritualistic burning at the stake that duplicated the deaths of the pre-Nicene martyrs. Why did Saunders and his fellow reforming martyrs adopt a stylized pattern of behavior similar in many respects to the Catholic ritual they abhorred and believed ineffectual for salvation?

By using the insights gleaned from psychological theory, it may be possible to shed some light on the phenomenon of Christian martyrdom and to understand why the most fervent and enigmatic of all religious types followed a rigid style of life and a ritualistic ceremony of death. Those insights suggest that the martyrs found it necessary to adopt an unbending form of ritualistic behavior to insulate themselves from the inner perturbations caused by their fear of martyrdom. Many were plagued by the suspicion that the actual motive in accepting martyrdom was suicide. Assuaging this fear required a

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<sup>1</sup> John Foxe, *The Acts and Monuments of John Foxe*, ed. J. Pratt, 8 vols. (London, 1877), 6: 613.

<sup>2</sup> *Ibid.*, 613.

systematic examination of conscience, an anguished soul-searching conducted according to definite rules. In contrast to normal, adaptive ritualistic behavior,<sup>3</sup> the ceremony devised by the martyrs was pathological, both in its sources and in its pattern: the source was what Erik Erikson has called "the weight of excessive guilt"<sup>4</sup> and the pattern was what Norman Cameron has described as "neurotic devotion to exact repetition, rigid uniformity, strict taboo, and severe self-punishment."<sup>5</sup>

Since the original studies by Sigmund Freud,<sup>6</sup> the connection between religious ceremony and obsessional neurosis has been scrutinized and debated.<sup>7</sup> Although theories regarding the precise nature of this defense mechanism and its tie to religion have varied, psychological analysts share many common assumptions about its dynamics. For one thing, scholars do agree that the phenomenon of obsessional compulsion is a system with its own discernable pattern of defense. Aggressive impulses lead to unconscious conflicts resulting in anxieties, which the obsessional neurotic seeks to reduce through useless but irresistible ritual and ceremony, through doubt and rumination, and through counterphobic devices. The inability to resolve conflict—love versus hate, right versus wrong, order versus disorder—creates

<sup>3</sup> For the best single compilation of materials on ritualistic behavior, see the study organized by Julian Huxley: "A Discussion on Ritualization of Behavior in Animals and Man," *Philosophical Transactions of the Royal Society of London*, ser. B, 251 (1966): 249–525. For other valuable studies, see in particular James D. Shaughnessy, ed., *The Roots of Ritual* (Grand Rapids, Mich., 1973); and Paul W. Pruyser, *Between Belief and Unbelief* (London, 1975). For the importance of ritualistic behavior in Tudor England, see Keith Thomas, *Religion and the Decline of Magic* (New York, 1971), 51–173.

<sup>4</sup> Erikson, "Ontogeny of Ritualization in Man," in Huxley, "Ritualization of Behavior," 345. Erikson distinguishes between healthy, entirely benign ritualization—such as the greeting ritual between mothers and infants—and guilt-induced, pathological rituals. He believes that healthy ritualization is creative, in that it "reveals the bond created by a reciprocal message of supreme adaptive importance"; *ibid.*, 337. As E. Shils notes, "Neurotic ritualization is a pattern of conduct in which the individual seeks to protect himself from dangerous impulses within himself and to undo acts of destruction instigated by such dangerous internal impulses"; Shils "Ritual and Crisis," in Huxley, "Ritualization of Behavior," 447. It is not, however, the purpose of this paper to discuss at any length normal ritualistic behavior, only to define the difference between these two modes. That the reformers deliberately chose martyrdom lends further evidence to their behavior as pathological. I believe that the best way to analyze this neurotic behavior is in psychological terms.

<sup>5</sup> Norman Cameron, *Personality Development and Psychopathology* (Boston, 1963), 374.

<sup>6</sup> For Freud's original study of the link between religion and compulsive behavior, see his short "Obsessive Actions and Religious Practices" (1907), in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, ed. James Strachey, 24 vols. (London, 1953–74), 9 (1954): 117–27. Freud elaborated upon this theme six years later in an expanded, more popular, study; see his "Totem and Taboo" (1913), in *Standard Edition*, 13 (1955): 1–161.

<sup>7</sup> For some of the more useful studies, see Theodor Reik, "Final Phases of Belief Found in Religion and in Obsessional Neurosis," *International Journal of Psycho-Analysis*, 9 (1930): 278–91, and *Dogma and Compulsion* (New York, 1951); Otto Fenichel, *The Psychoanalytic Theory of Neurosis* (New York, 1945); Hans Sachs, "The Transformation of Impulses into the Obsessional Ritual," *American Imago*, (1946): 67–74; Heinz Hartmann, *Essays on Ego Psychology* (New York, 1965); Anna Freud, *The Ego and the Mechanisms of Defense*, trans. Cecil Baines (New York, 1966); Leon Salzman, *The Obsessive Personality* (New York, 1968); and Marvin Rosen, "A Dual Model of Obsessional Neuroses," *Journal of Consulting and Clinical Psychology*, 13 (1975): 453–59. A survey of the literature indicates that since the 1930s there has been a sharp decline of studies connecting compulsive behavior and religion; see Alexander Grinstein, ed., *The Index of Psychoanalytic Writings*, 14 vols. (New York, 1956–75). Other recent work, however, indicates a slight increase in the number of psychological studies of religious phenomena from 1960 to the present. On this point, see William Saffady, "New Developments in the Psychoanalytic Study of Religion: A Bibliographic Review of the Literature since 1960," *Psychoanalytic Review*, 63 (1976): 291–99; and Donald Capps, Lewis Rambo, and Raul Ransohoff, eds., *Psychology of Religion* (Detroit, 1976).

ambivalence and a strong sense of guilt, particularly since the source of the conflict is hidden from its victim. The compulsive neurotic constructs a ritualistic pattern manifested by repetitive, orderly, and, at times, seemingly senseless behavior that he must perform slavishly to escape from anxiety. The original anxiety now gives way to a new anxiety over the performance of the exacting ritual, and punishment is meted out for failure to perform the ritual rather than failure to avoid the forbidden act.

THIS SYNDROME APPEARS IN THE ACTIONS OF MANY of the three hundred English Protestant reformers who, like Saunders, were burned at the stake between 1531 and 1558.<sup>8</sup> The Tudor martyrs could not totally repress their fear of a painful death by burning and, equally as important, their fear that the death they anticipated from the grace of God might, in fact, be the self-willed, self-inflicted path of suicide. They desired death, but agonized over whether they would face it bravely; they worried especially whether they might step over the boundary between martyrdom and suicide.<sup>9</sup> When a Roman Catholic sheriff questioned the reformer John Hooper about his lack of cooperation with the magistrates and his seeming desire for death, the martyr was quick to deny any attempt at self-destruction: "I wonder," remarked the sheriff, "that ye were so hasty and quick with the lord chancellor, and did use no more patience." Hooper carefully replied, "Master sheriff, I was nothing at all impatient, although I was earnest in my Master's cause, and it standeth me so in hand, for it goeth upon life and death, not the life and death of this world only but also the world to come."<sup>10</sup> Through the process of isolation, one of the components of the obsessive-compulsive system, Hooper had effectively deprived the original anxiety over suicide of its psychic energy. He could

<sup>8</sup> Approximately three hundred and fifty Protestant reformers were burned at the stake between the onset of the English Reformation and the accession of Elizabeth. For the best information on these figures, see British Library (hereafter BL), Lansdowne MS 817, ff. 168, 180; and BL, Harleian MS 421, ff. 10, 63. For a variety of approaches to the treatment of the martyrs along traditional lines, see A. G. Dickens, *The English Reformation* (London, 1968); G. R. Elton, *The Tudor Revolution in Government* (Cambridge, 1966); Maurice Powicke, *The Reformation in England* (London, 1941); James Gairdner, *Lollardy and the Reformation in England*, 2 vols. (London, 1908); and Philip Hughes, *The Reformation in England*, 3 vols. (London, 1951-54). For a study of seventeenth-century Puritan behavior that also has some interesting analyses of the Tudor martyrs, see Michael Walzer, *The Revolution of the Saints* (Cambridge, Mass., 1965). Several works concentrate upon individual martyrs; see, for example, Jasper Ridley, *Nicholas Ridley* (London, 1957), and *Thomas Cranmer* (London, 1966); William S. Brown, *The Life of Roland Taylor* (London, 1959); Alan Chester, *Hugh Latimer* (Philadelphia, 1954); and Robert E. Fulop, "John Firth (1503-1533) and His Relation to the Origin of the Reformation in England" (Ph.D. dissertation, University of Edinburgh, 1956). For studies of more than one martyr, see, among others, D. M. Loades, *The Oxford Martyrs* (London, 1970); E. G. Rupp, *Six Makers of English Religion, 1500-1700* (London, 1964); and William A. Clebsch, *England's Earliest Protestants, 1520-1535* (New Haven, 1964). For a more complete listing, see Seymour Byman, "The Psychology of Protestant Martyrdom in England, 1531-1538" (Ph.D. dissertation, Northwestern University, 1971), 235-60.

<sup>9</sup> As A. Alvarez has stated, "Suicide thinly disguised as martyrdom was the rock on which the Church had first been founded." Punishment for suicide was severe, both in this world and the next; the corpse was buried in the dark, unconsecrated ground outside the churchyard; and his earthly goods were confiscated by the state. Because the crown took over the goods of the suicide, there is a brief account of the suicide itself among the King's Bench and Star Chamber documents. Interestingly, a good number of the suicides appear to have been clerics. Note, for example, the case of a vicar who killed himself in Suffolk and whose possessions were seized by the crown; Public Record Office, London, STA 2 (1), f. 106.

<sup>10</sup> Foxe, *Acts and Monuments*, 6: 649.

calmly inform the magistrate that he was not eager to die, for, in his conscious mind, he was indeed “earnest yet not impatient.”

While in Newgate Prison awaiting her martyrdom, Anne Askew longed for death “as one that is bound towards heaven.” Significantly, she, like Hooper, attempted to suppress any thought that she might be hurrying the process by adding, “Written by me, Anne Askew, that neither wish death, nor yet fear his might.”<sup>11</sup> Nicholas Ridley, the intellectual leader of the Marian martyrs, also found it necessary to reassure himself that it was martyrdom, not suicide he sought: “To die in Christ’s cause is an high honor, to the which no man certainly shall or can aspire, but to whom God vouchsafeth that dignity; for no man is allowed to presume for to take unto himself any office of honor but he which is thereupon called to God.”<sup>12</sup> The key, then, was to tread the fine line between suicide and martyrdom.

To the reformers, true martyrs were agents of the Saviour witnessing His Passion and acting through God’s grace, whereas false martyrs were agents of the Antichrist acting upon their own perverse volition.<sup>13</sup> It was not enough to die bravely; the martyr had to be called by God. If bravery were the only criterion, then heaven would be filled with evil Anabaptists who were martyred in huge numbers throughout sixteenth-century Europe.<sup>14</sup> In an effort to furnish further proof that they were not committing suicide, the reformers projected their own desire for death onto their bitter enemies, the Anabaptists. That the Anabaptists died bravely proved only that they sought death, not salvation. As another martyr, Hugh Latimer, noted in a 1549 sermon before the boy king, Edward VI, “The Anabaptists that were burned here in divers towns in England went to their death even *intrepide*, as ye will say, without any fear in the world, cheerful. Well, let them go.”<sup>15</sup> Although Anabaptists and reformers were imprisoned together for heresy in Marian England, the reformers argued for the exclusion of the Anabaptists from God’s grace and thus illustrate what Erik Erikson has labelled the “pseudo species.”<sup>16</sup> “You say,” Bradford wrote to an Anabaptist incarcerated in a

<sup>11</sup> John Bale, “The Latter Examination of Anne Askewe” (1547), in Henry Christmas, ed., *Select Works of John Bale*, Parker Society (Cambridge, 1849), 209.

<sup>12</sup> Ridley, “A Piteous Lamentation of the Miserable Estate of the Church in England,” in Henry Christmas, ed., *The Works of Nicholas Ridley*, Parker Society (Cambridge, 1843), 77.

<sup>13</sup> A person who attacked God’s true martyrs was more likely than not to have his opinion disregarded; see, for example, the letter of Thomas Parry, a minister, to Foxe in 1571, some fifteen years after the death of a martyr named Julins Palmer. Parry and many others could not conceive of the martyrs as suicides. If Palmer did long for death to end his earthly troubles, then according to Parry “the martyr was guilty as well of inconstancy as also of willful casting away of himself”; BL, Harleian MS 416, f. 100. For my attempt to discover the hidden aspects of suicide and its tie to martyrdom, see “Suicide and Alienation: Martyrdom in Tudor England,” *The Psychoanalytic Review*, 61 (1974): 355–73.

<sup>14</sup> For a listing of some of the Anabaptist victims in Flanders, see A. L. E. Verheyden, *Le conseil des troubles: Liste des condamnés (1567–1573)* (Brussels, 1961). For a study of how these Anabaptists fit into the English framework, see Irvin Horst, *The Radical Brethren* (Nieuwkoop, 1972). And, for a good perspective from the reforming camp, see Gairdner, *Lollardy and the Reformation*.

<sup>15</sup> Latimer, “Fourth Sermon Preached before King Edward VI” (1549), in George Corrie, ed., *Sermons by Hugh Latimer*, Parker Society (Cambridge, 1844), 160.

<sup>16</sup> A reformer “becomes indoctrinated, then, with the conviction that his ‘species’ alone was planned by an all-wise deity, created in a special way cosmic event, and appointed by history to guard the only genuine version of humanity under the leadership of elect elites and leaders.” Erikson, *Identity, Youth, and Crisis* (New York, 1968), 298–99.



The martyr Hugh Latimer, bishop of Worcester from 1535 to 1539, who was burned at the stake on October 16, 1555. From a painting by an unknown artist, reproduced courtesy of the National Portrait Gallery, London.

different prison, "it hangeth partly upon our perseverance to the end; and I say, it hangeth only and altogether upon God's grace in Christ, and not upon our perseverance in any point 'for then were grace no grace.'"<sup>17</sup>

The reformers strongly believed that they alone comprised an exclusive coterie upon whom God's grace shone. T. S. Eliot's Fourth Tempter describes their aspirations:

What can compare with glory of Saints  
Dwelling forever in presence of God?  
What earthly glory, or king or emperor,  
What earthly pride, that is not poverty  
Compared with richness of heavenly grandeur?  
Seek the way of martyrdom, make yourself the lowest  
On earth, to be high in heaven.  
And see far off below you, where the gulf is fixed,  
Parched passions, beyond expiation.<sup>18</sup>

The martyrs engaged in a constant struggle against doubts of their own worthiness. With profound psychological insight, John Foxe, their sixteenth-century hagiographer,<sup>19</sup> observed and reported the pattern of this anxiety, guilt, and fear: "But this we see common among holy and blessed, how the more devout and godly they are, having the fear of God before their eyes, the more suspicion and mistrust they have of themselves." Foxe then noted the results of the "suspicion and mistrust": "Whereby it cometh to pass that often they are so terrified and perplexed with small matters as though they are huge mountains."<sup>20</sup> Today, this process would be described as the displacement of the original destructive impulses through an obsessive concentration—"a persistent, ritualized thought pattern"—that directed the martyrs' attention away from the distress of the original thoughts onto the ritual. Concomitantly, compulsions—"persistent ritualized behavior patterns"<sup>21</sup>—emerged to control any further doubt. This neurotic pattern grew more elaborate and more

<sup>17</sup> Bradford, "Letter to a Free Willer," Jan. 1, 1555, in A. Townsend, ed., *Writings of John Bradford*, Parker Society, 2 vols. (Cambridge, 1848), 2: 165. The reformers hated Anabaptists far more than Roman Catholics, who were the obvious enemies, responsible for the imprisonment and martyrdom of many of the faithful. One possible explanation for this phenomenon may be what Fenichel describes as "identification with the aggressor"; *Psychoanalytic Theory of Neurosis*, 480. Such "identification," or lack thereof, has been hotly debated in the massive literature on victim behavior in the concentration camps.

<sup>18</sup> Eliot, *Murder in the Cathedral*, part 1.

<sup>19</sup> Although he obviously championed the reformers, John Foxe accurately and diligently compiled his material. A rare exception, to my knowledge, involves the martyr John Cardmaker, for whom Foxe edited the testimony of John Rogers to delete evidence of Cardmaker's recantation; see BL, Lansdowne MS 389, f. 190. If one avoids the obvious embellishments of this hagiography, Foxe's *Acts and Monuments* is the most valuable source for this subject. For some of the more valuable works on Foxe's veritability, see V. N. Olsen, *John Foxe and the Elizabethan Church* (Berkeley and Los Angeles, 1973); J. F. Mozley, *John Foxe and His Book* (London, 1940); J. A. F. Thomson, "John Foxe and Some Sources for Lollard History: Notes for a Critical Appraisal," *Studies in Church History*, 2 (1962): 251–57; and A. G. Dickens, "Heresy and the Origins of English Protestantism," in John S. Bromley and Ernest H. Kossmann, eds., *Britain and the Netherlands*, 2 (Gronigen, 1964): 47–66.

<sup>20</sup> Foxe, *Acts and Monuments*, 7: 385. Freud contends that the line between the need to suffer and the need to persecute is fine indeed: "The more virtuous a man is, the more severe and distrustful is his conscious behavior, so that ultimately it is precisely those people who have carried saintliness furthest who reproach themselves with the worst sinfulness." "Civilization and Its Discontents" (1930), in *Standard Edition*, 21 (1961): 125.

<sup>21</sup> Salzman, *Obsessive Personality*, 10.



rigid with successive crises.<sup>22</sup> The ritual itself demanded much of the martyrs, for it imposed acts that were measured, precise, specified in great detail, and often quite repetitive. Through the exacting routine performance of these rituals, "the menace of dangerous spontaneity was gone,"<sup>23</sup> and thus the possibility of the emergence of objectionable thoughts was greatly reduced, if not eliminated. Should an unacceptable thought surface, a quick return to the ritual could once again bring reassurance.

Nicholas Ridley, for example, organized his external life rigorously, planning his day in precise detail:

He, using all kinds of ways to mortify himself, was given to much prayer and contemplation; for duly every morning, so soon as his apparel was done upon him, he went forthwith to his bedchamber, and there upon his knees, prayed the space of half an hour; which being done, immediately he went to his study, if there came no other business to interrupt him, where he continued till ten of the clock, and then came to the common prayer, daily used in his house. The prayers being done, he went to dinner, where he used little talk, except otherwise occasion by some had been ministered, and then was it sober, discreet, and wise, and sometimes merry, as cause required.

The dinner done, which was not very long, he used to sit an hour or thereabouts, talking, or playing at the chess: that done, he returned to his study, and there would continue until five of the clock at night, and then would come to common prayer, as in the forenoon: which being finished, he went to his supper behaving himself there as his dinner before. After supper recreating himself in playing at chess the space of an hour he would then return again to his study; continuing there till eleven of the clock at night, which was his common hour to go to bed, then saying his prayer upon his knees, as in the morning when he rose.<sup>24</sup>

In order to cope with doubt, Ridley carefully scheduled even his chess games, not to mention his prayers; it was imperative to follow an unchanging pattern—be it praying, studying, or sleeping. Ridley and his fellow martyrs suffered from what Søren Kierkegaard has called the "rising of the fever in the sickness of the self" and were forced to battle despair, "the sickness unto death."<sup>25</sup>

Many reforming martyrs surpassed even Ridley's compulsive inflexibility in their struggle against the temptations of doubt.<sup>26</sup> When they forsook worldly goods, ate sparingly, visited the aged and infirm, and devoted themselves to prayer and constant study of God's word, two English Protestant martyrs, Thomas Bilney and John Bradford, behaved in a manner similar to their sixteenth-century Roman Catholic counterparts, Ignatius Loyola and Teresa of Avila.<sup>27</sup> In order to train themselves in the act of suffering and to receive the

<sup>22</sup> Shils believes that "rituals are about crises, actual and anticipated"; "Ritual and Crisis," 447.

<sup>23</sup> Fenichel, *Psychoanalytic Theory of Neurosis*, 285.

<sup>24</sup> Foxe, *Acts and Monuments*, 7: 408.

<sup>25</sup> Kierkegaard, *The Sickness unto Death*, trans. Walter Lowrie (Oxford, 1941), 26.

<sup>26</sup> "Compulsion is for the neurotic as for the theologian an attempt to compensate for doubt. This attempt is made with the greater energy in proportion to the strength of the impulses against which faith has to defend itself." Reik, *Dogma and Compulsion*, 125.

<sup>27</sup> There is a striking similarity between the reformers and Loyola in his *Spiritual Exercises*, especially his first and second exercises on death. During the period of his spiritual enlightenment, Loyola's actions and

blessing of God, they constantly struggled against evil, real and imagined. "Good mother," Bradford counseled, "I wish to you forever God's peace in Christ. But this cannot be had or kept without war with ourselves, with the world, and with the devil."<sup>28</sup>

Although the earnest Protestant reformers ostensibly dismissed the Catholic emphasis on good works and physical deprivation as a direct path to salvation, they used these means to pave the way for their version of the true spirit. Like Saunders, Bilney—the first of the reformers to be burned—complained constantly of the ritualistic nature of the institutional church: "The authorities appointed me fastings, watching, buying of pardons, and masses." Yet, Bilney pursued rigidly formalized paths of his own. He established a fixed, daily schedule according to which he slept but four hours, ate only one small meal, visited the old and dying, mortified himself, and prayed constantly. Whenever the future bishop, Thomas Thirlby, Bilney's neighbor at Cambridge, played the recorder, Bilney went "straight to his prayer,"<sup>29</sup> fearing that music might distract him from his dogmatic pursuit of eternity. After months and years of exacting self-discipline, Bilney was able, on the eve of his martyrdom, to place his hand in the flame of a candle for such a long time that he burned off part of his finger. "What do you Master Bilney?" he was asked. "Nothing but trying my flesh by God's grace, and burning one joint, when tomorrow God's rods shall burn the whole body in the fire."<sup>30</sup> Adopting the same pattern as Bilney, George Eagles also attempted to "immerse himself therewithall against all necessities."<sup>31</sup> So familiar is this pattern of rigid austerity, that it is indistinguishable from a Roman Catholic hagiography.<sup>32</sup> Discipline was the means to keep the prospective martyr on the straight and narrow path to salvation.

John Bradford best illustrates the use of rigid ritual to attempt to eliminate the guilt associated with a furtive desire for death. Bradford's war with

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longings paralleled those of the reformers: "He persevered in his seven hours of prayer on his knees, getting up continually at midnight and doing all the other exercises already mentioned." But Loyola still had doubts about suicide: "While he had these thoughts, the temptation often came over him with great force to throw himself into a large hole in his room next to the place where he was praying. But realizing that it was a sin to kill oneself, he shouted again, 'Lord, I will do nothing to offend you,' repeating these words many times, as well as the previous ones." *The Autobiography of St. Ignatius Loyola*, trans. J. O'Callaghan (New York, 1974), 35. Both Loyola and the reforming martyrs were forced to increase the intensity of their "spiritual exercises" to cope with their doubts.

<sup>28</sup> Bradford to his mother, Feb. 24, 1554 (from the Tower of London), in Townsend, *Writings of John Bradford*, 2: 72.

<sup>29</sup> Foxe, *Acts and Monuments*, 4: 621. Bilney was the first reforming martyr, burned at the stake in 1531. Bilney "from the first" fell "into such a scrupulous holiness that he reckoned himself hounded so straitly to keep and observe the words of Christ to the very letter"; W. E. Campbell, *Erasmus, Tyndale, and More* (London, 1949), 11.

<sup>30</sup> Foxe, *Acts and Monuments*, 4: 652.

<sup>31</sup> Eagles did so much itinerant preaching that he was called "Trudgeover." And "his diet was so above measure spare and slender, that for the space of three years, he used for the most part to drink nothing but very water, whereunto he was compelled through necessity of the time of persecution: and after, when he perceived that his body, by God's providence, proved well enough with this diet, he thought best to inure himself therewithal against all necessities"; Foxe, *Acts and Monuments*, 8: 393–94.

<sup>32</sup> For a brilliant comparison of medieval hagiography as outlined in the *Legenda Aurea* with Foxe's *Acts and Monuments*, see Helen C. White, *Tudor Books of Saints and Martyrs* (Madison, Wisc., 1963).

himself was precipitated by a transgression he committed as a young man. While assisting the paymaster of the English forces, he embezzled a large sum of money. For several years, Bradford tried in vain to gain absolution through punishment, actually proposing at one point that he become a slave. He lived "in a world in which he cannot do the right thing. He is in an endless regress of wrong-right-wrong-right, doing-undoing-doing-undoing, undoing his undoing, etc."<sup>33</sup> In an effort to expiate his sins, which now included a good deal more than the embezzlement, Bradford devoted the last years of his life to study and prayer. Like Bilney, he allocated but four hours of his day to sleep lest it interfere with salvation. Sleep meant acknowledgement of physical limits and of lack of both physiological and psychological control over his own body.<sup>34</sup> Like Saunders and Loyola, Bradford used spiritual exercises:

He used to make unto himself an ephemeris or a journal, in which he used to write all such notable things as wither he might see in that book the signs of his smitten heart. For if he did not see or hear any good in that man, by that sight he noted the want thereof in himself, and added a short prayer, craving mercy and grace amend. If he did hear or see any plague or misery, he noted it as the thing procured by his own sins, and still added *Domine Miserie Mei*. Lord, have mercy upon me. He used in the same book to note such evil thoughts as did rise in him; as of envying the good of other men, thoughts of unthankfulness, of not considering God in his works, of hardness and unsensibleness of heart when he did see other moved and affected. And thus he made to himself and of himself a book of daily practices and repentance.<sup>35</sup>

Bradford's obsessive-compulsive pattern became more evident as martyrdom approached. Although Bradford was "sore troubled . . . by dreams"<sup>36</sup> throughout his life, his dreams grew more and more frightening while in prison for they exposed his unconscious fears.<sup>37</sup> Could he withstand the pain

<sup>33</sup> R. D. Laing, "Ritualization and Abnormal Behaviour," in Huxley, "Ritualization of Behaviour," 333. Bradford's act of embezzlement assumed gigantic proportions and, as Sandor Rado has described, the transgression literally consumed its victim; see Rado, \_\_\_\_\_ in William Gaylin, ed., *The Meaning of Despair* (New York, 1968), 70-95. For a further and more complete discussion of Bradford's guilt and attempts at expiation, see my "Guilt and Martyrdom: The Case of John Bradford," *Harvard Theological Review*, 68 (1975): 305-31.

<sup>34</sup> Salzman, *Obsessive Personality*, 62.

<sup>35</sup> Thomas Sampson, Preface to Bradford, *Two Notable Sermons Made by That Worthy Martyr of Christ Master John Bradford* (London, 1574), third unnumbered page.

<sup>36</sup> Foxe, *Acts and Monuments*, 7: 146.

<sup>37</sup> The reformers were in a quandary as to the efficacy of their dreams. They felt dreams were a device heavily used by the Papists, and should be discredited. For example, the martyr Thomas Cranmer, former Archbishop of Canterbury, attacked the worth of dreams: "yet dreams seen of many men in their sleep, as though it were of them that are departed hence, have deceived, destroyed, and overthrown many men"; John Cox, ed., *Miscellaneous Writings and Letters of Thomas Cranmer*, Parker Society (Cambridge, 1846), 43. Yet, before their execution, the martyrs had significant dreams, which many thought had to be a message from God. This ambivalence comes out in Foxe's discussion of the dream that the martyr Cutbert Sympton had before his execution: "Within two hours after, about eleven of the clock toward midnight, the said Cutbert (whether being in a slumber, or being awake I cannot say) heard one coming in, first opening the outward door, then the second, after the third door, and so looking in to the said Cutbert . . . saying, 'Hah!' unto him. and departed again." Foxe then has a long digression on the worth of these dreams: "Some I see will not believe it; some will deride the same; some also will be offended with setting forth things of that sort uncertain . . . Again, neither am I ignorant that the papists, in their books and legends of saints, have their prodigious visions and apparitions of angels, of our Lady, or Christ, or other saints; which as I will not admit to be believed for true, so will they ask me again, why should I then more require these to be credited

of death? Might he be a suicide rather than a martyr? Shortly before his death, Bradford dreamed about "how the chain for his burning was brought to the Compter-gate and how the next day, being Sunday, he should be had to Newgate, and on the Monday after burned in Smithfield." Awakening at three in the morning, Bradford roused his cellmate "and told him his unquiet sleep, and what he was troubled withal." But a mere recounting of the dream was not enough; only the customary rites could accomplish the needed catharsis. "Master Bradford rose out of the bed, and gave himself to his old exercise of reading the prayer that always he had used before." The ritual appeared to help, for that night "at dinner, according to his accustomed manner, he did eat his meat and was very merry." He now attempted to suppress any more fears of suicide and pain, telling himself that he was indeed a legitimate martyr and would be able to overcome the pain. When Bradford discovered that he would burn the following day, he appeared to be a happy man. "With that master Bradford put off his cap, and lifting up his eyes to heaven, said, 'I thank God for it; I have now looked for the same a long time, and therefore it cometh not now to me suddenly, but as a thing waited for every day and hour; the Lord make me worthy thereof.'"<sup>38</sup>

But he was still troubled. He had not mastered his anxiety since the sources were hidden from him by his own defenses. What was Bradford to do when he had spent a good part of his life attempting to suppress his longing for death, and, just at the point at which he was able to realize his desire (a desire he dared not recognize), dreams he could not control told him of his unconscious wishes? His recourse was an even greater reliance upon compulsive ritual to eliminate these objectionable feelings. In order to master his anxiety and undo his pain of guilt, he prayed in his customary manner, undergoing his spiritual exercises. But this time, since the anxiety was so great, he expanded the ritualistic mechanism to include communal prayer. Now, he needed the reassurance of others to maintain his self-esteem.<sup>39</sup> On the night before his death, "half a dozen of his friends more, with whom all the evening he spent the time in prayer and other good exercise," helped him through his penultimate ritual.<sup>40</sup>

**YET THE STAKE REMAINED.** After imprisonment the martyrs had even more reason to prove that their approaching deaths were neither selfish nor sui-

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to them, than their of us." Foxe moves further, stating that he is only reporting the dream, that the reader does not have to believe it. Foxe, *Acts and Monuments*, 8: 456-57. Ironically, today since the studies of Freud on dreams, we are more likely to believe in their importance than were the reformers. Raymond DeBecker calls dreams "material for action"; *The Understanding of Dreams*, trans. Michael Heron (New York, 1968), 13. Pruyser notes that in dreams one comes to terms with pleasures missed as well as with pains experienced or feared; *Between Belief and Unbelief*, 206. For the best analysis of the importance of dreams in Tudor England, see Thomas, *Religion and the Decline of Magic*, 128-32.

<sup>38</sup> Foxe, *Acts and Monuments*, 7: 147. In a letter to Bradford shortly before his martyrdom, Ridley was quite concerned about the manner of his death. Ridley had readied himself for burning and had heard a rumor that Hooper was "hanged, drawn, and quartered," the penalty for treason. Ridley was very relieved to discover that the report was unfounded. See Christmas, *Works of Nicholas Ridley*, 379.

<sup>39</sup> On this need, see Fenichel, *Psychoanalytic Theory of Neurosis*, 293.

<sup>40</sup> Foxe, *Acts and Monuments*, 7: 147.

cidal, but "the whole burnt sacrifice which the Lord delighteth in."<sup>41</sup> As the Marian martyr, Richard Roth, noted in a congratulatory letter to a fellow martyr in 1555, "O how blessed are you in the Lord, that God hath found you worthy to suffer for his sake: with all the rest of my dear Brethren and Sisters known and unknown. O be joyful even unto death, fear it not, saith Christ, for I have overcome death."<sup>42</sup>

How, then, would Bradford and his fellow martyrs face death? Would their deaths be called or sought? Until the final moment at the stake, the martyrs were not certain that their sacrifices were called by God. In order to convince themselves that their calling was indeed authentic, they extended their daily rituals to include the death stand. They made certain that their deaths at the stake paralleled that of the Saviour and the early Christians. Were they different from St. Ignatius, who in A.D. 107 expressed his longing for pain and martyrdom?

I am writing to all the Churches, and I give injunctions to men, that I am dying willingly for God's sake, if you do not hinder it. I beseech you, be not an unseasonable kindness to me. Suffer me to be eaten by the beasts, through whom I can attain to God. I am God's wheat, and I am ground by the teeth of wild beasts that I may be found pure bread of Christ. Rather entice the wild beasts that they may become my tomb, and leave no trace of my body, that when I fall asleep I be not burdensome to any. Then shall I be truly a disciple of Jesus Christ, when the world shall not even see my body. Beseech Christ on my behalf, that I may be found a sacrifice through these instruments.<sup>43</sup>

A millenium and a half later, this desire for death was iterated by a Marian martyr. At the conclusion of an impassioned prayer before his burning in 1555, Nicholas Sheterdon stressed the necessity of his sacrifice.

O Father, I do not presume unto thee, in mine own righteousness; no, but only in the merits of thy dear Son my Saviour. For the which excellent gift of salvation I cannot worthily praise thee, neither is any sacrifice worthy, or to be accepted with thee, in comparison of our bodies mortified, and obedient unto thy will. And now, Lord, whatsoever rebellion hath been, or is found in my members, against thy will, yet do I here give unto thee my body to the death, rather then I will use any strange worshipping, which I beseech thee accept at my hand for a pure sacrifice. Let this torment be to me the last enemy destroyed, even death, the end of misery, and the beginning of all joy, peace and solace; and when the time of resurrection cometh, then let me enjoy again these members then glorified, which now be spoiled and consumed by the fire. O Lord Jesus receive my spirit into thy hands. Amen.<sup>44</sup>

By identifying themselves with the pre-Nicean martyrs, the reformers sought assurance that their own deaths were legitimate. Since Jesus and the early

<sup>41</sup> Hooper to John A. Lasco, 1553. "May he grant what he commands, and then command things yet more painful if it seemeth him good." Hastings Robinson, ed., *Original Letters Relative to the English Reformation*, Parker Society, 2 vols. (Cambridge, 1846), 1: 101.

<sup>42</sup> Miles Coverdale, *The Letters of the Martyrs* (1564; reprint ed., London, 1837), 526.

<sup>43</sup> "Ignatius to the Romans," iv, 1-2, in *The Apostolic Fathers*, trans. Kirsopp Lake, 2 vols. (London, 1965), 1: 231.

<sup>44</sup> Foxe, *Acts and Monuments*, 7: 313. That the reformers slavishly imitated the pre-Nicean martyrs suggests a need for legitimacy of their neuroses. This rigid imitation of past martyrs, therefore, adds to the necessity of a psychoanalytic interpretation of their actions.



martyrs did not commit suicide, surely the Tudor martyrs were also innocent of taking their own lives. As the true heirs of Christ, they played their roles to perfection, intoning the proper words and gestures they believed Jesus might have used during His martyrdom. There was no need to be original; they merely had to re-enact the battle against Satan that had been fought a millennium and a half before.

Because Christ and His disciples encountered only sorrow and pain for their endeavors, the reforming martyrs, in their quest for identity,<sup>45</sup> felt it wrong to anticipate a better fate, for if they could escape tribulation, they might also escape Heaven. Their imitation must be total. "John Baptist was beheaded; Christ was crucified; the apostles were killed," Latimer preached in 1552. "This was the reward for their labor. So all the preachers shall look for none other reward; for no doubt they must be sufferers."<sup>46</sup> Steadfastness amidst the flames would eradicate any pain and cleanse any guilt. John Philpot not only preached the maxim "the Cross is now the way to heaven"<sup>47</sup> but literally practiced its promise throughout his martyrdom.

According to Freud, the ceremonial and imitative acts of the obsessive-compulsive personality are performed as a "defence against temptation and partly as a protection against the misfortune expected." The temptation of the martyrs was an early death, and the misfortune anticipated was punishment for suicide. The martyrs thus faced, in Freud's terms, "an insoluble conflict" continually requiring "fresh mental efforts . . . to counterbalance the constant forward pressure of the impulse."<sup>48</sup> The most important moment of their lives, the time of extreme duress, occurred as the reformers prepared themselves for a heroic death in imitation of the Saviour. "Now I go to practice that which I have preached," wrote Bradford shortly before his death. "Now I am climbing up the hill: It will cause me to puff and blow before I come to the cliff."<sup>49</sup> But the steep and ragged cliff of the stake had to be surmounted if heaven's doors were to open. The stake became as much a struggle against self-doubt as a battle against the idolatries of their opponents.

With the imminence of immolation, the reformers' fears of a painful death loomed large. No longer could they theorize about how they might perform, about whether they might recant. As Ridley aptly phrased it, "they must turn

<sup>45</sup> Margaret Mead believes that the "ritual act serves to re-create to some extent the intense emotion experienced in the past"; "Ritual and Social Crisis," in Shaughnessy, *The Roots of Ritual*, 90. That need provides one of the reasons why Foxe compiled his book in England and Crespin constructed his martyrology in France. On the latter, see Donald R. Kelley, "Martyrs, Myths, and the Massacre: The Background of St. Bartholomew," in Alfred Soman, ed., *The Massacre of St. Bartholomew* (The Hague, 1974), 183. In his pioneering study in psychohistory, Erik Erikson examined Martin Luther's quest for identity and meaning; see Erikson, *Young Man Luther* (New York, 1962). The martyr George Marsh expressed this quest for identity while awaiting martyrdom: "And if we suffer anything for the kingdom of heaven and righteousness sake, we have the prophets, Christ, the Apostles, and Martyrs for an ensample to comfort us; for they did all enter into the kingdom of heaven at the strait gate and narrow way that leadeth unto life, which few do find." See Coverdale, *Letters of Martyrs*, 523.

<sup>46</sup> Corrie, *Sermons by Hugh Latimer*, 468.

<sup>47</sup> Philpot to Mistress Ann Hartipole, 1555, in Robert Eden, ed., *The Examinations and Writings of John Philpot*, Parker Society (Cambridge, 1842), 251.

<sup>48</sup> Freud, "Obsessive Acts," 124. The impulse is self-inflicted death.

<sup>49</sup> Bradford to Mistress Warcup, Feb. 7, 1555, in Townsend, *Writings of John Bradford*, 2: 185.



or burn."<sup>50</sup> To overcome the pain of death was no mean accomplishment. The description of the death of John Lambert, the Henrician reformer who died in 1538, indicates the degree of pain each martyr could anticipate:

For after that his legs were consumed and burned up to the stumps, and that the wretched tormentors and enemies of God had withdrawn the fire from him, so that but a small fire and coals were left under him, then two that stood on each side of him, with their halberts pitched him upon their pikes as far as the chain would reach. . . . Then he, lifting up such hands as he had, and his fingers ends flaming with fire, cried unto the people in these words, "None but Christ, none but Christ"; and so being let down again from their halberts, fell into the fire, and there ended his life.<sup>51</sup>

Few reformers sat idly, waiting until the last moment to discover whether or not they had the mettle to face the ordeal. They devised a new series of counterphobic measures to cope with the anxiety of a painful death. The martyrs actively participated in ritualistic death stands; they were thus not only able to allay their anxieties, but even make the prospects of painful death seem pleasurable.<sup>52</sup> John Philpot's ordeal at the stake was replete with the counterphobic devices employed by many martyrs. Upon reaching his final place of suffering, he kissed the stake and then spoke to the crowd with assurance. "Shall I disdain to suffer at this stake, seeing my Redeemer did not refuse to suffer a most vile death upon the cross for me?"<sup>53</sup> In kissing the stake Philpot evidently overcame his fear of it—in confronting the stake, the symbol of his fears, and kissing it, he removed the sting, just as Tudor children were expected to kiss the rod with which they were punished.<sup>54</sup>

In 1543, three Henrician martyrs, Henry Filmer, Robert Testwood, and Anthony Peerson, told their supporters surrounding the stake "not to be moved at their afflictions, for it was the happiest thing that ever came to them."<sup>55</sup> Because they were certain that everlasting life would be gained by losing their earthly bodies in this painful but "happy" ordeal, the three martyrs began their ritual by celebrating their forthcoming entrance into heaven with a drink to everlasting health in the world to come.<sup>56</sup> As they

<sup>50</sup> Ridley to Thomas Cranmer, 1535, in Christmas, *Works of Nicholas Ridley*, 363. As was true of the traitor, the martyr was expected to behave bravely and with style at death. For a discussion of the etiquette and forms involved, see Samuel Y. Edgerton, "Maniera and the Mannaia: Decorum and Decapitation in the Sixteenth Century," in Franklin W. Robinson and Stephen Nichols, eds., *The Meaning of Mannerism* (Hanover, N.H., 1972), 67. This toughness at death follows the fifteenth-century pattern; see Johann Huizinga, *The Waning of the Middle Ages*, trans. F. Hopman (Harmondsworth, Middlesex, 1955), esp. chaps. 1, 2, 11.

<sup>51</sup> Foxe, *Acts and Monuments*, 5: 236.

<sup>52</sup> Robert J. Kastenbaum has recently noted that people who seek a career in the health professions tend to have a greater fear of death than do people in general—they use their work as a counterphobic device; Kastenbaum, *Death, Society, and Human Experience* (St. Louis, 1977), 66. And René Laforgue has discovered that in many patients anxiety was very sensual and pleasurable; Laforgue, "On the Erotization of Anxiety," *International Journal of Psycho-Analysis*, 11 (1930): 312.

<sup>53</sup> Eden, *Examinations and Writings of John Philpot*, 161.

<sup>54</sup> For the best contemporary account of childhood punishment patterns in Tudor England, see Bartholomew Batty, *The Christian Mans Closet*, trans. W. Louth (London, 1581), STC 1591. For a fuller analysis of this practice, see my "Melancholy and Child Raising Practices in Tudor England," *Journal of Psychohistory*, forthcoming (Summer, 1978). I am grateful for the advice of Lloyd deMause on this point.

<sup>55</sup> Foxe, *Acts and Monuments*, 5: 493.

<sup>56</sup> This practice in part also parallels the ancients; Socrates made a libation out of the hemlock.

drank from a communal pot of wine, they delivered their last words ecstatically. Filmer embraced the stake and told his brethren to be merry and to “lift up your hearts unto God; for after this sharp breakfast, I trust we shall have a good dinner in the kingdom of Christ our Lord, our Redeemer.”<sup>57</sup>

The embracing of the stake can also be interpreted according to the dynamics of the process of introjection, which is yet another counterphobic mechanism of the obsessive-compulsive.<sup>58</sup> When Peerson approached the stake, he embraced it and cooed, “now welcome mine own sweet wife, for this day shall thou and I be married together in the love and peace of God.”<sup>59</sup> Peerson thus attempted to merge with the stake and, in so doing, relieve the hurt he both anticipated and dreaded. Even more striking in his incorporation of the stake was Laurence Saunders. “He fell to the ground and prayed: he rose up again, and took the stake to which he should be chained, in his arms, and kissed it, saying, ‘Welcome the cross of Christ, welcome everlasting life!’ and being fastened to the stake, and fire put to him, full sweetly he slept in the Lord.”<sup>60</sup> By embracing the symbol of their destruction, which was transformed into the symbol of immortality, the martyrs would merge themselves with Christ, overcome death, and gain salvation.

The ritual of the stake assumed a definite pattern: prayer, forgiving enemies, consoling friends, donning the death garments, caressing the stake, fixing one’s eyes on heaven, the raising of hands throughout the fire, and finally death. The death stand of Cicely Ormes, who was martyred in 1557, is replete with these devices:

Then she came to the stake, and laid her hand on it, and said “Welcome the cross of Christ.” Which being done, she, looking on her hand, and seeing it was blacked with the stake, wiped it upon her smock; for she was burnt at the same stake that Simon Miller and Elizabeth Cooper were burned at. Then, after she had touched it with her hand, she came and kissed it, and said, “My soul doth magnify the Lord, and my spirit rejoiceth in God my Saviour.” And in so saying, she set her hands together right against her breast, casting her eyes and head upward; and so stood, heaving up her hands by little and little, till the very sinews of her arms did brast in sonder, and then they fell.<sup>61</sup>

The kissing of the stake and the ritual position of awaiting heaven with hands “together against her breast” were the protective measures she devised to “tame death.”<sup>62</sup> The actions of Ormes, repeated over and over again by other

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Ritualistic devices, in a sense, protected these martyrs. As Anna Freud noted, through the obsessional the anxiety was bound, guilt was diminished, and punishment was granted; *The Ego and the Mechanisms of Defense*, 52.

<sup>57</sup> Foxe, *Acts and Monuments*, 5: 493.

<sup>58</sup> Introjection can also enable the compulsive to regain the omnipotence that the child lost while growing; see Fenichel, *Psychoanalytic Theory of Neurosis*, 147.

<sup>59</sup> Foxe, *Acts and Monuments*, 5: 493.

<sup>60</sup> *Ibid.*, 6: 628.

<sup>61</sup> *Ibid.*, 8: 429.

<sup>62</sup> See P. Ariès, *Western Attitudes towards Death* (Baltimore, 1974), 8. William Saffady views much of Tudor England as dominated by a need to control emotions and falling under the system of obsession-compulsion; see his “Fears of Sexual License during the English Reformation,” *History of Childhood Quarterly*, 1 (1973): 89–97.

martyrs, transformed the Catholic *mea culpa* into reforming ritual. This compulsive pattern of repetition that the celebrant followed at death was scrupulously observed down to the last moment at the stake. After Rowland Taylor “had prayed, he went to the stake, and kissed it, with his hands folded together and his eyes toward heaven.”<sup>63</sup> When the somewhat reluctant martyr, John Cardmaker, was called to test his faith,<sup>64</sup> “his prayer being ended,” he proceeded to “put off his clothes unto his shirt.” Then he went to the stake, “kissed it sweetly,” and “so gave himself to be bound most gladly.”<sup>65</sup>

The conquering of physical pain at the stake was the culmination of the ritualistic act of imitation. Since the reformers assumed that Christ overcame pain and uncertainty in His passion, they were also determined not to flinch from the pain of the fire. Now, in the midst of the intense flames of martyrdom, the reformers realized that they had conquered their fear of pain, and, because their imitation of Christ was complete, they dispelled any remaining doubts over suicide. Some of the martyrs had succeeded in conquering their anxiety to such a degree that they transformed their pain into pleasure and were, as Philpot urged, “joyful under the cross.”<sup>66</sup> James Bainham told his onlookers that he felt no agony, “for in this fire I feel no more pain, than if I were in a bed of down.”<sup>67</sup> Richard Bayfield, another early reforming martyr, was thankful at the stake that he was delivered from the Antichrist, and was happy to die “to be in heaven with Jesus Christ and the church triumphant forever.” With a deliberate display of indifference to pain, Bayfield placed his right arm in the fire when his left arm burned off “and continued in prayer to the end without moving.”<sup>68</sup> Thomas Cranmer placed the hand in the fire that most offended God,<sup>69</sup> while Ridley and Hooper were roasted over a slow fire. Martyr Thomas Tomkins, asked by Bishop Edmund Bonner if he could endure the fire, replied by placing his hand over a flame without flinching for some time<sup>70</sup> and thereby emulated Bilney, who had burned his finger to the joint in preparation for the ordeal.

To the reforming martyrs, their fates had been enacted a millennium and a half before by the martyrs of the early church. Simply by altering the names of the heroes and villains, past and present became one: the Roman Empire became the Roman Catholic Church, and the reformers died at the hands of evil Papists just as their brethren had died at the hands of Roman emperors. An early martyr whom the reformers used as a model was Cyprian, bishop of Carthage, beheaded in 258. Not only did Cyprian demonstrate his willingness

<sup>63</sup> Foxe, *Acts and Monuments*, 6: 699.

<sup>64</sup> Cardmaker evidently wavered before his martyrdom until he was pressured by the martyr Laurence Saunders: “For Cardmaker after he had obtained the company of Saunders was thought to have gathered more courage to defend the truth.” BL, Harleian MS 425, f. 67.

<sup>65</sup> Foxe, *Acts and Monuments*, 7: 82.

<sup>66</sup> Eden, *Examinations and Writings of John Philpot*, 226.

<sup>67</sup> Foxe, *Acts and Monuments*, 4: 705.

<sup>68</sup> *Ibid.*, 688.

<sup>69</sup> “And forasmuch as my hand offended, writings contrary to my heart, my hand shall first be punished therefore; for, may I come to the fire, it shall first be burned.” BL, Harleian MS 417, f. 90.

<sup>70</sup> James Gairdner, *The English Church in the Sixteenth Century* (London, 1902), 361.

to die by blindfolding himself, but he added a superb touch: "When he came to the place of execution, he ordered his people to give fifteen gold pieces to the headsman for his pay."<sup>71</sup> John Bradford duplicated this generosity when he "gave money to every servant and officer of the house" where he had been incarcerated.<sup>72</sup> Bradford completed his imitation at the stake, for there he combined Cyprian's final grandiose gesture with the counterphobic act implicit in caressing the stake. The ceremony consisted of three parts: Bradford's kissing of the stake, his gift of clothing to a servant, and his outstretched arms at death. John Foxe described the scene:

... and then master Bradford took a faggot in his hand, and kissed it, and likewise the stake. And when he had so done, he desired of the sheriff that his servant might have his raiment: "for," said he, "I have nothing else to give him: and besides that, he is a poor man." The sheriff said he should have it. And so forthwith master Bradford did put off his raiment, and went to the stake: and, holding up his hands, and casting his countenance up to heaven, he said thus, "O England, England, repent thee of thy sins, repent thee of thy sins. . . ."<sup>73</sup>

Execution by burning, the sentence for heretics in England, provided precious moments before the flames were lit when the martyrs felt free to perform the entire ritual of the ceremony of imitation.

The Bible itself offered many different precedents that the reformers could follow in their death ritual. St. Paul, for example, presented one alternative: "I desire then that in every place the men should pray, lifting holy hands without anger or quarreling."<sup>74</sup> If a man should pray everywhere without anger, what better place than at the stake. The three Christians who were burned at Tarragona in Spain in 259 personified Paul's words as they "stretched out their arms in token of the Lord's victory, praying to him till they gave up their souls."<sup>75</sup> The Tudor martyrs transformed Paul's words and the actions of these three early Christians into their own death ritual. Cicely Ormes "heaved up her hands" little by little until she died, while John Bradford held up his hands as he cast his eyes upward toward heaven. Shortly before his martyrdom in 1555, Thomas Haukes, an English gentleman, made a pact with his friends "that in the midst of the flames he would show them some token" so that they might know "whether the pain of such burning were so great that a man might not therein keep his mind quiet and patient." After the fire was lit, Haukes continued to pray but finally "his speech was taken away by the violence of the flames." Yet, even with "his fingers consumed by the fire," he did not forget his promise; he "reached up

<sup>71</sup> Jacobus de Voragine, *The Golden Legend*, trans. Granger Ryan and Helmut Rippergor (New York, 1969), 550–51. For an able analysis of the deaths of Cyprian and other pre-Nicean martyrs, see W. H. C. Frend, *Martyrdom and Persecution in the Early Church* (Oxford, 1965).

<sup>72</sup> Foxe, *Acts and Monuments*, 7: 147.

<sup>73</sup> *Ibid.*, 194.

<sup>74</sup> 1 Timothy 2:8 (RSV).

<sup>75</sup> Donald Atwater, *The Dictionary of Saints* (Harmondsworth, Middlesex, 1965), 165; and Frend, *Martyrdom and Persecution*, 321.

his hands burning on a light fire" and "struck or clapped them three times together."<sup>76</sup>

The raising of the hands, which became formalized into ritual for many of the English martyrs, was therapeutic. Like the seemingly meaningless rituals of the compulsive, the reformers' ritual of religious imitation helped dispel the demons of anxiety. After the "fire was set unto him," William Flower invoked the holy number of the trinity to ward off the Devil. He "cried with a loud voice, 'O the son of God, have mercy upon me! O the Son of God receive my soul!' three times until his speech was taken from him."<sup>77</sup> After he was no longer able to call upon oral ritual, he continued the ceremony even though "first his right hand being held up against the stake was stricken off" leaving only his left hand intact. He finished, nevertheless, by "lifting up, notwithstanding, his stump with the other arm, as long as he could."<sup>78</sup> But Christopher Wade, a linen weaver from Kent, performed the most remarkable feat. When the fire was thoroughly kindled, "he was heard by no man to speak" but was left "still holding his hands up over his head together toward heaven, even when he was dead and altogether roasted." Surely, thought the spectators, this was a sign from God; it must be God who "no less wonderfully sustained those hands which he lifted up to him for comfort in his torment."<sup>79</sup>

To die bravely in accordance with the established ceremony not only helped dispel any doubts the martyrs might have entertained regarding suicide, but established the standards that determined who was a martyr.

Wherein this in him is to be noted, that when he had been a long time tormented in the fire without moving, having his flesh so broiled and puffed up, that they which stood before him underneath could see the chain wherewith he was fastened, and therefore supposed no less but he had been dead; notwithstanding suddenly he spread abroad his arms, saying, "Father of heaven, have mercy upon me!" and so yielded his spirit unto the hands of the Lord.<sup>80</sup>

George Marsh is a case in point. Because of Marsh's brave actions, "many of the people said that he was a martyr and died marvellous patiently and godly."<sup>81</sup> To follow the prescribed ceremony indicated to other reformers that

<sup>76</sup> Foxe, *Acts and Monuments*, 6: 114-15.

<sup>77</sup> *Ibid.*, 76. Paul Pruyser believes that the repetition of words brings power and notes that "the first ritualistic feature of religious speech is repetition"; Pruyser, *A Dynamic Psychology of Religion* (New York, 1968), 133. Fenichel states that the favorite number of repetition of words is usually even; *Psychoanalytic Theory of Neurosis*, 288. In Nicholas Ridley's farewell letters from prison, he illustrates this pattern of even repetitions; for this pattern, see especially Christmas, *Works of Nicholas Ridley*, 427. It would also seem likely that in the religiously oriented milieu of Tudor England, the holy number three would often be employed, as Flower did, to ward off the dangerous fears. The name of Jesus, then, would be repeated to ward off hostile spirits, both internal and external. Bradford used an even pattern; see Foxe, *Acts and Monuments*, 7: 194. Salzman stresses that the ritual may be expiatory, or supplicatory with repetitive formulae to influence the gods and control anxiety; *Obsessive Personality*, 56. On this means to assuage the anger of the gods, also see Reik, "Religion and Obsessional Neurosis," 289.

<sup>78</sup> Foxe, *Acts and Monuments*, 7: 76.

<sup>79</sup> *Ibid.*, 320-21.

<sup>80</sup> *Ibid.*, 53.

<sup>81</sup> *Ibid.*

the martyr was indeed called by God and that, by following the same pattern at the stake, they too would receive the rewards of everlasting life.

Nicholas Ridley and Hugh Latimer presented what was perhaps the most spectacular finale of Tudor martyrdom. The death stands of these two reforming leaders were choreographed from past martyrdoms, their dress and last words an imitation of past witnesses. Ridley, representing the past glory of his position as bishop of London, came to the stake dressed in “a fair black gown furred and faced with foins, such as he was wont to wear being bishop, and a tippet of velvet furred likewise about the neck.” And Ridley gave gifts in imitation of Cyprian: “some napkins, some nutmeg, and rases of ginger, his dial, and such things as he had about him, to everyone that stood next to him.” Latimer, on the other hand, represented the present suffering of the true church in the austerity of his dress. He appeared in “a poor Bristol frieze frock all worn, with his buttoned cap, and a kerchief on his head, all ready to the fire, a new long shroud hanging over his hose.” Thus dressed, Ridley and Latimer demonstrated “on the one side, the honor they sometime had and, on the other, the calamity whereunto they were fallen.” And, since Latimer’s role was to represent the poverty of Christ, he “gave nothing.” As the fire was kindled, Latimer was given an opportunity to utter his now-famous words: “Be of good comfort, master Ridley, and play the man. We shall this day light such a candle, by God’s grace, in England, as I trust shall never be put out.”<sup>82</sup> Even these inspirational words were not fully original, for they echoed the mysterious words spoken in 155 at the death of the early Christian martyr, Polycarp: “Now when Polycarp entered into the arena there came a voice from heaven! ‘Be strong, Polycarp and play the man.’ And no one saw the speaker, but many of our friends who were there heard the voice.”<sup>83</sup>

WHATEVER THE JUDGMENT OF THE MARTYRDOMS MAY BE, it should not be forgotten that the sixteenth-century milieu of these martyrs was quite different from our own. When the rewards of the next world—the eternal world—appeared so great, when earth in the center of the universe was seen as a huge dung heap collecting refuse, when heaven itself was believed by all to be at the top of fixed stars, how natural it must have been for men and women to want to soar from this world to the next through death. The reforming martyrs earnestly believed that a fiery death would launch their voyage from this sinful, uncertain sphere to the best of all possible worlds. The flames would eradicate the torments of the martyrs: the sin, the guilt, the doubt, the

<sup>82</sup> *Ibid.*, 8: 547–50.

<sup>83</sup> “Martyrdom of Polycarp,” ix, 1, in *Apostolic Fathers*, 2: 323; and Eusebius, *H.E.*, iv, 15–18. Frend suggests that Eusebius attempts to show that Polycarp did not seek martyrdom but “had placed himself in God’s hands”; *Martyrdom and Persecution*, 216. Polycarp’s martyrdom was very important for future hagiography; his martyrdom composed by Marcianus became a cornerstone of the Church. On the place of Polycarp’s martyrdom, see Hans Lietzmann, *A History of the Early Church*, trans. B. L. Woolf (Cleveland, 1953), 137–38.



anxiety.<sup>84</sup> Confidently, they anticipated the rewards of eternity that they fully expected after faithfully performing their exacting ritual. The martyrs believed that death was indeed a small price to exact for such rewards.

Let us not then fear death, which can do us no harm, otherwise then for a moment to make the flesh to smart; for that our faith, which is surely fastened and fixed unto the word of God, telleth us that we shall be anon after death in peace in the hands of God, in joy, in solace, and that from death we shall go straight unto life.<sup>85</sup>

<sup>84</sup> The flames provided the needed purgation, a very important quality in the sixteenth century. Natalie Zemon Davis sees the Huguenots in France as rejecting holy water and incense for purification but, following Deuteronomy, accepting fire as a sacred means for purification; Davis, "The Rites of Violence: Religious Riot in Sixteenth-Century France," *Past & Present*, no. 59 (1973): 82. Gaston Bachelard also stressed the purification of fire as expressed in literature; see his classic study, *The Psychoanalysis of Fire*, trans. Alan Ross (Boston, 1968).

<sup>85</sup> Ridley, Farewell Letter from Prison, 1555, in Christmas, *Works of Nicholas Ridley*, 426.

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*AHR Forum*  
American Imperialism:  
The Worst Chapter in Almost Any Book

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JAMES A. FIELD, JR.

A FEW MONTHS AGO the mail brought a copy of a new textbook on American diplomatic history. Feeling some obligation to the publisher, I gave it the standard check and read through the chapter on the 1880s and 1890s—"The New American Spirit." Like so many such chapters, it failed the test. Disappointed again, I crossed the hall to ask a learned colleague what he thought was predictably the worst chapter in any general history of American foreign relations. "The worst?" he asked, and then answered without a pause, "The one on the end of the nineteenth century."

This "worst chapter" may be summarized somewhat as follows:

The publication of Charles Darwin's *Origin of Species* gave rise to some new American mutations called John Fiske, Josiah Strong, Alfred T. Mahan, and Brooks Adams. Unlike their ancestors, they were all racists and wanted battleships and naval bases. At the same time the American churches, desirous of saving souls, demanded political control of "native peoples." In the closing years of the century American farmers began to ship more wheat and cattle to "Europe and other places." Standard Oil and other companies were investing overseas. An "avalanche" of books, articles, editorials, and speeches consequently descended upon the American people, sending it into the "psychic crisis" of the 1890s and turning the country to "populism and jingoism." The result of all of this was the arrival of a Samoan chief in search of a treaty, a riot in a Valparaiso saloon, a revolution in Hawaii, and a naval development program that bore fruit in a new secret weapon, a twenty-inch gun, which was test-fired in the direction of Whitehall in 1895. The "logical outcome" of this new spirit (described in the next chapter) was "The War with Spain" and the annexation of the Philippines as part of a policy of "insular imperialism" aimed at the markets of China.

This curious narrative, which in mildly variant forms appears to have gained wide acceptance, represents the product of some fifty years of histori-

cal construction by a number of architects and builders. Surprisingly, the foundations still reflect the handiwork of Julius W. Pratt (although John A. Hobson and Charles A. Beard have crept back into the cellar); the eclectic superstructure derives from the efforts of, among others, Richard Hofstadter, William A. Williams, Walter LaFeber, and Thomas J. McCormick. Although the mansion was originally designed to house the 1880s and 1890s, the carpenters in recent years have added wings that extend backward in time to provide lodging for William H. Seward and forward to house Vietnam. The result of this architectural agglomeration is an inverted Whig interpretation of history, differing from its predecessor primarily in that now the children of darkness triumph over the children of light. But there remains the same insistence on seeing the past through the prism of the present, the same perceptions of false continuities and imputations of sin, and the same tendentious impact on generalization and abridgment.<sup>1</sup> Works of this genre have a certain utility, no doubt, for classroom discussion of how and how not to write history, but they also raise serious questions. How should we grapple with this segment of our past? How escape the conventional formulations so uncritically and tediously passed from article to article, book to book, and text to text?

MUCH OF THE LITERATURE ON THE 1890s suffers from a number of common failings. First, the approach is too rational. Chance (or the unexpected), which plays so important a part in the life of the individual, seems unacceptable in the life of the nation: these authors simply will not remember the *Maine*. Events must have their antecedent philosophers and strategists and must also, apparently, flow logically from previous intentions. Since the United States did entangle itself in Asia in 1898, these requirements have led to a backward approach to history and to the transformation of a record of almost total lack of accomplishment—in the search for naval bases, in Hawaiian annexation, in the construction of Chinese railroads, and in reviving the merchant marine, reforming the consular service, or revising the tariff—into evidence of an overwhelming wave of imperialism.<sup>2</sup> Second, the picture is too unitary. The use of such terms as “America,” “American,” and “United States” to describe both public and private doings imposes a deceptive

<sup>1</sup> Julius W. Pratt, *Expansionists of 1898: The Acquisition of Hawaii and the Spanish Islands* (Baltimore, 1936); Richard Hofstadter, *Social Darwinism in American Thought* (Boston, 1955); William A. Williams, *The Tragedy of American Diplomacy* (Cleveland, 1959); and *The Roots of the Modern American Empire* (New York, 1969); Walter LaFeber, *The New Empire: An Interpretation of American Expansion, 1860–1898* (Ithaca, N.Y., 1963); and Thomas J. McCormick, *China Market: America's Quest for Informal Empire, 1893–1901* (Chicago, 1967). Herbert Butterfield, *The Whig Interpretation of History* (London, 1931), 1–7, 11, 15, 21–26, 87–89, 110–12, *passim*. For a convenient brief analysis of the literature on American imperialism, see Göran Rystad, *Ambiguous Imperialism* (Lund, Sweden, 1975), 12–18.

<sup>2</sup> Equally, this backward approach to history has led to much emphasis on unimportant and unsuccessful figures (Perry McD. Collins, General James H. Wilson, Wharton Barker, and the like) and to a corresponding neglect of far more effective entrepreneurial frontiersmen (Cyrus W. Field, Henry Meiggs, Charles J. Harrah, William R. Grace, James A. Scrymser, Minor W. Keith, John Hays Hammond) who were active in areas little affected by the events of 1898.

solidity upon a very mixed bag of phenomena, confuses the governmental and private sectors, underplays the many overseas Americans who showed small interest in government support and none in territorial expansion, and obscures regional variation. Mapping late nineteenth-century American activity abroad produces a whole variety of overlays—economic, strategic, cultural, philanthropic, entrepreneurial—that are by no means geographically coincident. Third, the treatment is excessively ethnocentric. Everything that happens is attributed to the purposeful action of the United States, and the philosophers and strategists must all be American. But while the America of the 1890s possessed a large and influential package of skills and resources, both its strengths and its aims were limited. Indeed, with the recent history of the big influence of small allies available to reinforce that of the Cuban junta and the Hawaiian Annexation Club, it could perhaps be argued that the United States has been as much or more the used as it has the user, and that much of its involvement in the outer world has come in response to Macedonian cries.<sup>3</sup> Fourth, the discussion takes small account of time, distance, costs, or technological feasibility. Words do duty for things and presumed intentions for actual capabilities. Finally, the wrong questions are often asked. In the case of Hawaii, for example, the interesting problem is surely not why it was annexed in 1898, but rather why so “natural” a development was so long delayed. The answer is that ruling circles in the United States did not much want to annex Hawaii, unless, perhaps, to pre-empt annexation by others; contrariwise, influential folk in the islands ardently desired to annex the United States but, with only a small and feeble country at their disposal, had to wait for special circumstances before they could manage it.<sup>4</sup>

MOVING FROM THE GENERAL TO THE PARTICULAR, a brief look at various aspects of the treatments of “American imperialism”—ideological, bureaucratic, semantic, geographical, and technological—may help to separate what is useful from what is not. The presumed necessity for events to follow logically from what preceded, the importance scholars give to words, and the happy fact that quotations to support almost any argument can be rummaged out of the grab-bag of the past have led many historians to fall in love with ideology. Hence, the chapters on “The New American Spirit” and, hence, the emphasis on Darwin and on something called “Social Darwinism.” From the *Origin of Species*, it is suggested, came a mental climate that spawned oppressive capitalists at home and promoters of oppressive imperialism abroad. But there are problems here. The kind of activist government required for imperial ventures appears to have had small appeal for capitalists, and few seem to have busied

<sup>3</sup> Acts 16: 9–24. The modern reader of this text, observing how Paul became aware of the cry out of Macedonia and what befell him when he answered it, will sense ambiguities which seem to have eluded our missionary-minded forefathers.

<sup>4</sup> Thomas A. Bailey, “The United States and Hawaii during the Spanish-American War,” *AHR*, 36 (1931), 552–60; Merze Tate, *The United States and the Hawaiian Kingdom* (New Haven, 1963), and *Hawaii: Reciprocity or Annexation* (East Lansing, Mich., 1968); and William A. Russ, Jr., *The Hawaiian Revolution* (Selinsgrove, Pa., 1959), and *The Hawaiian Republic* (Selinsgrove, Pa., 1961).

themselves in the cause. The standard ideologists who are alleged to have infected the American people with the disease of Darwinist expansionism were few in number and of doubtful leverage, and the standard quotations from their works are selective and unrepresentative. One should not, it would seem, quote Fiske, Strong, Mahan, Adams, and the rest without having read their works.

Strong's main concerns in the 1880s, for instance, were clearly focused on the country's internal problems. As a high-minded Social Gospeller he worried mightily about Mormonism, Catholicism, drink, and tobacco; and his single chapter on the outer world appears to have been designed to emphasize what was at stake at home. There was, in any case, nothing new in 1885 about his ideas on the triumph of Anglo-Saxondom, Christianity, and civil liberty that would postulate "a new American spirit"; similar sentiments had been voiced in the 1840s by Hollis Read, another missionary author.<sup>5</sup> Fiske's general attitude was strongly antimilitary, and his recipe for the world's future, far from being one of conquest, was essentially that of world federalism, another old American idea with roots reaching back to the eighteenth century.<sup>6</sup>

With Mahan the case is somewhat different: invariably invoked, he is seldom quoted, doubtless because his thought was focused on hemispheric defense—"America is our sphere"—and helpful quotations about transpacific expansion are hard to find.<sup>7</sup> It may, indeed, be time for a collective rereading

<sup>5</sup> Josiah Strong, *Our Country*, ed. Jurgen Herbst (Cambridge, Mass., 1963); Dorothea R. Muller, "Josiah Strong and American Nationalism," *JAH*, 53 (1968): 487-503; Frederick Merk, *Manifest Destiny and Mission in American History* (New York, 1963), 239-46; and Hollis Read, *The Hand of God in History* (Hartford, 1849). Strong's much-quoted vision of the Anglo-Saxons of North America pressing down upon the southern continent appears to have been merely the conventional wisdom of the day: projecting the historic growth rates of the English- and Spanish-speaking populations of the New World into the twentieth century, the *Britannica* had recently foreseen for the latter a future confined to the hill country, like the Welsh. *Encyclopaedia Britannica*, 9th ed., 1: 716-17.

<sup>6</sup> John Fiske, *American Political Ideas Viewed from the Standpoint of Universal History* (New York, 1885), 101-52, and *The Destiny of Man Viewed in the Light of His Origin* (Boston, 1884); Merk, *Manifest Destiny*, 239; and Milton Berman, *John Fiske: The Evolution of a Popularizer* (Cambridge, Mass., 1961), 138-40. For some early expressions of the federal idea, see the sources cited in James A. Field, Jr., *America and the Mediterranean World* (Princeton, 1969), 13-15, 23-24.

<sup>7</sup> Robert Seager II and Doris D. Maguire, eds., *Letters and Papers of Alfred Thayer Mahan* (hereafter Mahan, *Letters and Papers*), 3 vols. (Annapolis, 1975), 2: 443, *passim*; and Alfred T. Mahan, *The Interest of America in Sea Power* (Boston, 1897), 261, 265, and *The Problem of Asia* (Boston, 1900), 7-8. This hemispheric emphasis was lasting: Mahan's war college lectures of the late 1880s, only slightly revised for later publication, focus on isthmus and Caribbean, and consider the Hawaiian Islands in reference to the West Coast. Mahan, *Naval Strategy* (Boston, 1911), *passim*. But historians have not scrupled to invent what they have failed to discover, and one may marvel at the words that have been put into the captain's mouth. Philip S. Foner has stated that in an article in *Harper's Monthly* in October 1897, Mahan called for "expansion of American economic activity in Latin America and Asia," urged aggressive moves into the markets of the Far East, and saw "a clear connection between the Caribbean and the vast market of China. . . , the coaling and cable station system in the Ladrões and Samoa, the Philippines . . ."; "Why the United States Went to War with Spain in 1898," *Science and Society*, 32 (1968): 57-58. This simply is not true. Mahan's article, "Strategic Features of the Gulf of Mexico and the Caribbean Sea," discussed what the title promised and nothing more; reprinted in Mahan, *Interest of America in Sea Power*, 271-314. For perhaps the best extended discussion of Mahan's influence, see William E. Livezey, *Mahan on Sea Power* (Norman, Okla., 1947). But see, *per contra*, Peter Karsten, "The Nature of 'Influence': Roosevelt, Mahan, and the Concept of Sea Power," *American Quarterly*, 23 (1971): 585-600. And, for negative evidence, see John D. Long, *The New American Navy* (New York, 1903); and Harry Thurston Peck, *Twenty Years of the Republic, 1885-1905* (New York, 1906).

of Mahan's pre-1898 writings, although how many will wish to work at length through what Admiral Castex described as a style "particulièrement abstraite et soporifique" is open to question. But such an exercise would find little "imperialism." The feeling that the United States should begin to look outward, "righteously but not with feeble scrupulosity," is matched by the finding that it was disinclined to do so: if, in fact, imperialism was spreading "like wildfire," Mahan did not know it. There is little stress on the expansion of the merchant marine and no concern for projection of power outside the hemisphere. Sea control, it is clearly stated, will never again be as important as during the eighteenth-century wars for empire. Emphasis is given the "aggressive restlessness" of the Europeans, as evidenced in overseas competition, and its implications for the Monroe Doctrine; the danger of possible collision with Britain, Spain, or Germany is raised; some concern is evinced about the awakening Orient. If there is a focus, it is on the strategic importance of the isthmus, and on the consequent need for protective offshore bases and a smallish but efficient navy, competent to defend against the detachments a European power might send against the United States. But the general effect is cloudy, and certainly no clear "imperialistic" plan emerges: reviewing *The Interest of America in Sea Power*, the British army officer George Sydenham Clarke, a great admirer of *The Influence of Sea Power upon History*, found it "extremely difficult to extract a definite meaning from his pages."<sup>8</sup>

It seems possible that one reason for the perdurability of these authors in treatments of this period is their use of the terms "Anglo-Saxon" and "race," in the context of what the twentieth century has done to words like these. But in 1885 there was nothing either very novel or very naughty in this usage. The term "Anglo-Saxon" as an umbrella word for British and Americans was some forty years old. The word "race," as any dictionary will show, can be used in either a biological or a cultural sense, and few contemporaries would have thought that references to the "French race," the "Spanish race," or the "Anglo-Saxon" or "English-speaking" race had anything to do with the vexed question of whether the biological races of mankind numbered five as suggested by Johann Blumenbach, eleven as argued by Charles Pickering, or four as proposed by Thomas Huxley.<sup>9</sup> If talk of the achievements of the "Anglo-Saxon race" seems nowadays a little out of style, they could hardly have been overlooked in the 1880s. In extent of imperial sway no country equalled Great Britain, and in territorial growth none the United States. The Industrial Revolution, commenced in England, had leapt the Atlantic to produce a still greater economic expansion. On the scales of civil liberty and representative democracy none could match the British and Americans. Nor had any other societies deployed so many missionaries and mechanics to carry the gift of salvation, whether by conversion or modernization, to those who dwelt in darkness.

<sup>8</sup> Mahan, *Influence of Sea Power*, *passim*, and *Interest of America in Sea Power*, *passim*, and *Letters and Papers*, *passim*; James A. Field, Jr., "Alfred Thayer Mahan Speaks for Himself," *Naval War College Review*, 29 (1976): 47-60; and Clarke, "Captain Mahan's Counsels to the United States," *Nineteenth Century*, 43 (1898): 292-300.

<sup>9</sup> *Encyclopaedia Britannica*, 9th ed., 2: 112-13.



On the question of the “Anglo-Saxon race,” these authors—though often confusing in their usage—were anything but racist in the anthropological sense. “I use the term,” Strong wrote, “somewhat broadly to include all English-speaking peoples.” In his approval of the “commingling of races” that was taking place in America, he was at one with the American editor of the *Riverside Natural History* who, in this same *annus mirabilis* of 1885, argued the superiority of mixed stocks, with Darwin and with Mahan (who was very clear on the mixed origins of both British and Americans and who came to include the Japanese within the “European family”). Even Brooks Adams, who fussed a good deal about the international Jewish banking community, thought the survival of civilizations dependent upon the infusion of “barbarian blood.”<sup>10</sup>

Since contemporaries, like later historians, may well have read selectively, one should perhaps test the assertions that these authors were influential. Josiah Strong’s *Our Country* sold a good many copies over the years, but the book got short shrift from the establishment press and no one has yet named a policy maker influenced by Strong’s ideas.<sup>11</sup> John Fiske’s histories decorated everyone’s library shelves, but *American Political Ideas*, which contained his piece on “‘Manifest Destiny,’” was not one of his fastest sellers, and the allegation that he delivered this chapter as a speech to a receptive president and cabinet rests on a misreading of the evidence.<sup>12</sup> Brooks Adams published nothing on American foreign policy until the summer of 1898.<sup>13</sup> Captain Mahan no doubt deserves some credit for the increased popularity of navies and the arguments for battleships, but his principal effort to influence policy misfired badly: throughout his life he remained convinced that his annexationist article on Hawaii had brought about his transfer by the Cleveland administration from writing and lecturing at the Naval War College to a greatly undesired tour of sea duty.<sup>14</sup>

<sup>10</sup> Strong, *Our Country*, xxi, 202, 210–11; John S. Kingsley, ed., *The Riverside Natural History* (Boston, 1885), 6: 471–72; Charles Darwin, *The Descent of Man* (New York, 1888), 142; Mahan, *Problem of Asia*, 147–48, 193; Brooks Adams, *The Law of Civilization and Decay* (New York, 1898), xi, 362–65, 383; and Arthur Beringause, *Brooks Adams* (New York, 1955), 119, 122, 141–42, 178–79. Howard Mumford Jones has some sensible comments on nineteenth-century Anglo-Saxonism (as does Mr. Dooley, whom he quotes); Jones, *The Age of Energy* (New York, 1973), 200–11. On the “racism” of Theodore Roosevelt, see Howard K. Beale, *Theodore Roosevelt and the Rise of America to World Power* (Baltimore, 1956), 26–34. The rhetoric of Fiske and Strong is not our rhetoric, no doubt, but toploftiness is not imperialism. Given the world of the 1880s, there seems nothing very tendentious in their formulations. The basic idea, indeed, would shortly prove persuasive to Frenchmen; see Edmond Demolins, *A quoi tient la supériorité des Anglo-Saxons* (Paris, 1897).

<sup>11</sup> Muller’s work should, one would think have settled this question once and for all; “Josiah Strong and American Nationalism,” 487–503. Also see Merk, *Manifest Destiny*, 236–37.

<sup>12</sup> Compare John Spencer Clarke, *The Life and Letters of John Fiske*, 2 (Boston, 1917): 166–69, 172; with Richard Hofstadter, *Social Darwinism in America* (Philadelphia, 1944), 177–78; and Berman, *John Fiske*, 134–35, 141–42.

<sup>13</sup> For a list of his writings, see Thornton Anderson, *Brooks Adams, Conscientious Conservative* (Ithaca, N.Y., 1951), 229–31. Foner has stated that Adams’s *Law of Civilization and Decay* argued the health of imperialism and “advocated American control of the Western hemisphere and economic dominance of Asia”; “Why the United States Went to War,” 45. It does not. The book starts with the ancient world and ends with nineteenth-century Britain; its subject is “the origin, rise and despotism of the gold-bug”; the United States is barely mentioned. See, in default of the work itself, Anderson, *Brooks Adams*, 50–72; Beringause, *Brooks Adams*, 117–28; and Ernest Samuels, *Henry Adams, The Major Phase* (Cambridge, Mass., 1964), 127–30.

<sup>14</sup> Mahan, *Letters and Papers*, 2: 138, 3: 299.

If the claimed impact of the so-called “imperialist” tracts on either the American people or important figures in government tends to dissolve on inspection, one would still presume that the publicists themselves, if they were in fact concerned with empire, would have turned excitedly to work as the Cuban crisis deepened. Alas, the picture is far otherwise. In the spring of 1898 John Fiske, preoccupied with the preparation of lectures on science and religion, was distressed by the possibility of war.<sup>15</sup> Josiah Strong, like the good reformer he was, was busy founding the League for Social Service.<sup>16</sup> Brooks Adams left Washington for his home in Quincy in August 1897, sailed for Europe in September, and did not return until spring; early in 1898, as war seemed imminent, he wrote his brother Henry, “I am in despair to have this silly business forced on us, where we can gain neither glory nor profit.”<sup>17</sup> Ten weeks after the navy had begun a precautionary redeployment, six weeks after the publication of the De Lôme letter, five weeks after the sinking of the *Maine*, and four weeks after the *Oregon* had been started east, the secretary of the navy appointed Captain Mahan the United States representative at a Florentine celebration in honor of Vespucci and Toscanelli, and on March 26, 1898 America’s best-known strategic thinker sailed for Naples.<sup>18</sup> Theodore Roosevelt’s old racist teacher, John W. Burgess, prostrated by the events of 1898, turned out to be antiwar, anti-imperialist, and anti-Roosevelt. The greatest of the Social Darwinists, William Graham Sumner, thought the upshot of the war disastrous—*The Conquest of the United States by Spain*.<sup>19</sup>

Yet, whatever the facts about these much-quoted intellectuals, it might still be possible that the 1890s were years of burgeoning sentiment for imperialism

<sup>15</sup> Clarke, *John Fiske*, 2: 472–73; and Berman, *Fiske*, 251–52. In December 1897 Fiske wrote an historical introduction for a book by his son-in-law on the Cuban insurrection. Although Anglo-Saxon attitudes pervaded the discussion of the “absolute despotism” of the Spanish Empire and the “Satanic” Inquisition, a strong sympathy for the Cuban cause led to the argument that “for the sake of Cuba’s best interests, it is to be hoped that she will win her independence without receiving from any quarter, and especially from the United States, any such favors as might hereafter put her in a position of tutelage. . . .” Grover Flint, *Marching with Gomez* (Boston, 1898), xv, xxvii.

<sup>16</sup> Muller, “Josiah Strong and American Nationalism,” 485–503; and *Dictionary of American Biography*, 18: 150–51.

<sup>17</sup> Beringause, *Brooks Adams*, 161–62; and Frederic Cople Jaher, *Doubters and Dissenters* (New York, 1964), 173. In these circumstances the elevation of Brooks Adams, whose expansionist efforts were unknown to the generation of Julius W. Pratt, to the rank of principal pre-1898 imperialist, is surely one of the most remarkable developments in recent historical writing. William A. Williams has described him as “something of the chairman of an informal policy-planning staff for the executive department in the years from 1896 to 1908,” and Walter LaFeber has stated that Adams, along with Mahan, “exerted more direct influence on policy makers than did any of the other intellectuals,” and that “throughout 1897 and early 1898 Adams” saw great opportunities in the coming war with Spain, entertained such important people as Cushman Davis, Lodge, and Mahan at dinners in Washington, and was hailed as a prophet by leading figures of the McKinley administration. Williams, “The Frontier Thesis and American Foreign Policy,” *Pacific Historical Review*, 24 (1953): 387; Williams, *America in a Changing World* (New York, 1978), 38; and LaFeber, *The New Empire*, 80, 84–85. Foner has added that Henry Adams “worked actively in Washington in favor of intervention in Cuba”; “Why the United States Went to War,” 45. But Henry was abroad from April 1897 to November 1898 and learned of the loss of the *Maine* while boating on the Nile; Samuels, *Henry Adams*, 185, 589.

<sup>18</sup> Mahan, *Letters and Papers*, 2: 543, 548.

<sup>19</sup> Pratt, *Expansionists of 1898*, 11; and Sumner, *Conquest of the United States* (Boston, 1899).

and concern for the China market. If so, one would suppose this could be documented by a bureaucratic analysis and by lists of people who really mattered—presidents, secretaries of state or navy, senior military officers, Wall Street giants—who were energetic in the cause. The task has not been easy, notwithstanding the recent boom for Benjamin Harrison and his secretary of the navy, Benjamin F. Tracy, and their very ineffective efforts to gain a naval base on Hispaniola.<sup>20</sup> Faced with a foot-dragging Senate, and lacking both suitable movers and shakers and evidence in party platforms and presidential messages, the convention has been to fall back on Fiske and Strong, on the Omaha *Bee*, on the social thought of Harry Thurston Peck, and on the improbable (or at least highly irrational) desire of farmers for a China market to absorb the surplus. In place of identifiably influential persons or pressure groups, there has developed a usage which suggests the virtue of semantic analysis. Persuasion is attempted by incantation: by the use of reiterated statements that “the imperialists” did this and “the imperialists” did that, that “imperialism spread like wildfire,” and that under the prodding of the remarkably faceless “imperialists” the American people went island-grabbing.<sup>21</sup> What generally escapes notice is that, when the anti-imperialists come on stage, there is no problem at all in producing a formidable roster of distinguished citizens—Carl Schurz, E. L. Godkin, William Vaughan Moody, David Starr Jordan, Andrew Carnegie (who briefly thought of personally purchasing and liberating the Philippines but who, regrettably for the exceptionalism of American history, failed to act on this impulse), Mark Twain, William Jennings Bryan—the list goes on and on.<sup>22</sup>

With semantic analysis, in addition to the question of the unidentified “imperialists,” other problems appear. It may be proper to speak figuratively of a unitary “American people” after the sinking of the *Maine*, but, given the politics of 1892 and 1896, earlier unqualified use of the phrase seems questionable. A similar problem of precision arises in relation to that naughty minor-

<sup>20</sup> Walter R. Herrick, *The American Naval Revolution* (Baton Rouge, La., 1966); and B. F. Cooling, *Benjamin Franklin Tracy: Father of the Modern American Fighting Navy* (Hamden, Conn., 1973). For a useful list of influential individuals who did not want war in 1898, see Wayne S. Cole, *An Interpretive History of American Foreign Relations* (Homewood, Ill., 1968), 268–69.

<sup>21</sup> See, for example, Herrick, *Naval Revolution*, 24, 60, 69, 84–87, 90, 103–07, 196, 220. On the “imperialism” of one of the few identified “imperialists,” Henry Cabot Lodge, see John A. S. Grenville and George B. Young, *Politics, Strategy, and American Diplomacy* (New Haven, 1966), 201–38. In general this literature appears to depend heavily upon code-words (“imperialism,” “new empire,” “commercial empire,” “hegemony,” “commercial hegemony of the world,” etc.) whose effects seem more emotive than heuristic. For an attractively written example, see Ernest N. Paolino, *The Foundations of American Empire: William Henry Seward and U.S. Foreign Policy* (Ithaca, N.Y., 1973). Paolino has argued that plans for the rationalization of the world’s coinage and the stringing of the international telegraph were intended to bring about (but how?) American “world commercial hegemony”; see, in particular, pages 6, 14, and 25. LaFeber’s extensive repetition of the title phrase—*New Empire*—appears to have persuaded many that the concept existed in its own right long before Brooks Adams published his own book with that title in 1902.

<sup>22</sup> For a representative treatment, see the chapter on “Empire Beyond the Seas” in John M. Blum *et al.*, *The National Experience* (3d ed.; New York, 1973), 491–506. The terms “imperialist,” “imperialists,” and “imperialism” appear twenty-one times and eight pre-1898 “imperialists” are named (Strong, Burgess, Fiske, and Mahan constitute half of the contingent); the corresponding terms “anti-imperialist,” “anti-imperialists,” and “anti-imperialism” are used four times and nineteen individuals are identified.

ity of "Americans in Hawaii" who were busily conspiring for annexation. For one thing not all were: the most strongly nativist prime minister of the latter years of the Hawaiian kingdom, Walter M. Gibson, had grown up in New York and New Jersey, and the biggest sugar grower, Claus Spreckels, came to oppose annexation. For another, some of these "Americans" had been born and raised in the islands. Is nationality inalienable? Was Albert Gallatin always a Swiss and Andrew Carnegie always a Scot? Was Spreckels a German or an American or a Hawaiian? Despite the obvious goodheartedness of those who inveigh against the Hawaiian annexationists as against the apostles of Anglo-Saxonism, their own usage seems curiously racist.

Another context in which it is useful to remember that words are not things, and to look through the word to the phenomenon for which it stands, is in discussion of navies and naval bases. The creation of the new American navy in the last two decades of the century was an event of undoubted importance, which might, at first glance, seem to provide bureaucratic evidence for an expansionist policy. But navies can be designed for various purposes: there is no necessary connection between naval building and commercial expansion or colonization, and battleships do not equate with empire.<sup>23</sup> The military characteristics of a naval force—the capabilities designed into it—should give some indication of its intended missions; and the reiterated statements of the secretaries to the effect that the New Navy was purely defensive, intended only "as a police force for the preservation of order and never for aggression," are substantiated by the instrument that they created.<sup>24</sup>

Given the limitations of American industry, reinforced as they were by American naval tradition, the first phase of naval revival perforce involved the construction of that "fleet of swift cruisers to prey on the enemy's commerce" desired by Mahan. Here the remoteness of the United States from important European trade routes and from such centers of interest as the River Plate, Valparaiso, and Honolulu strongly affected ship characteristics and by 1890 had brought about the design of cruisers of remarkable coal capacity and endurance. Such ships would seem to have been ideally suited for distant ventures; but, once domestic sources of armor and of castings for gun barrels became available, the emphasis switched to the completion of heavily armed monitors for harbor defense and to construction of "sea-going coastline battleships," restricted in range by congressional enactment and in draft by the hydrography of gulf ports, to contest the control of North American waters. Still further expression of these defensive concerns developed during the 1890s in the authorization of a score of short-range torpedo boats and in the recommendation of 1897—based on the argument that "the traditional policy

<sup>23</sup> Leaving aside the European great powers, eight nations (Argentina, Brazil, Chile, China, Denmark, Greece, Spain, Turkey) had acquired battleships by 1893, while the United States still had none. At least on paper China and Spain were clearly superior in cruiser strength as well. None of these countries showed much sign of expansionist or imperialist policy. *Report of the Secretary of the Navy*, 1893.

<sup>24</sup> See, for example, *Report of the Secretary of the Navy*, 1889, 1890, 1891, 1893, 1897, and the excerpt from Cleveland's annual message of December 1885 quoted by the secretary of the navy in 1893 and 1896.

of our Navy has been a defensive one"—that future units of this type be designed to use the new oil fuel available only at home.<sup>25</sup>

In the early years of the building program the emphasis on commerce raiding and defense of home waters had left the ancient mission of maintaining a presence on such distant stations as the China coast and the River Plate to the wooden ships of the Old Navy. But, as these decayed beyond repair or were washed or blown ashore, replacements became necessary. Over a period of ten years, beginning in 1885, authorization was secured for a number of new gunboats for this service; and, since coaling stations in these far-off regions existed neither in fact nor in contemplation, the specifications for four of the class of 1895 called for "full sail power." Finally, it may be noted, the New Navy was not, as seems sometimes assumed, the product of a presumably expansionist Republican Party: by 1896 the two Cleveland administrations had gained authorization for half again as much tonnage as had those of Arthur and Harrison.<sup>26</sup>

Related to the naval building question is "the search for bases." Unquestionably, the coming of steam raised serious problems of range and endurance for the world's navies, but solutions to the coal problem varied as widely as the intended employment of the forces to be coaled. A rented site for a coal pile at Yokohama, Honolulu, or Pago Pago was one thing; a defended position like Gibraltar, Aden, or Hong Kong was quite another. When considering the meaning of such terms as "base" and "coaling station," it is well to be as precise as possible, and precision in chronology is also desirable. One should not, for example, use the "magnificent naval base at Pearl Harbor" as evidence of vigorous transpacific expansionism twenty years before anyone dredged the mouth of the Pearl River.<sup>27</sup> And, if the search for

<sup>25</sup> On commerce raiders, see Mahan, *Letters and Papers*, 1: 593. Mahan emphasized the importance of defense against blockade and bombardment; *Influence of Sea Power*, 83-87. So did Theodore Roosevelt in his review of the book; *Atlantic*, October 1890. Also see *Report of the Secretary of the Navy, 1893, 1896*; and George T. Davis, *A Navy Second to None* (New York, 1940), 34-35, 86-100. Admiral David D. Porter had urged the need for torpedo boats in 1887; their importance was emphasized by the secretary in 1889 and 1892. On the question of oil fuel, see *Report of the Bureau of Steam Engineering, 1897*. Shortly after completion of his term as secretary, Hilary A. Herbert noted that battleship design had been predicated on the assumption of employment close to home, pressed the need for torpedo boats, emphasized the vulnerability of coastal cities and coastal shipping, and pointed to the possibility of collision with Great Britain, with Spain over Cuba, or with Japan over the Hawaiian Islands; "A Plea for the Navy," *Forum*, 24 (1897): 1-15.

<sup>26</sup> *Report of the Secretary of the Navy, 1891, 1895, 1896*; and Davis, *Navy Second to None*, 25-27. For a convenient listing of naval units with dimensions, armament, coal capacity, and dates of authorization and commissioning, see the *Report* for 1897. Mahan's concern about the state of the navy had led him to support Cleveland in 1884; *Letters and Papers*, 1: 574, 592, 624. The first Cleveland administration negotiated the contracts which created the domestic gun and armor-forging capacity. It seems hardly necessary to debate the proposition that so costly, continuing, and bipartisan a creation as the New Navy was the product of the status anxiety of junior officers; see Lloyd C. Gardner, Walter F. LaFeber, and Thomas J. McCormick, *Creation of the American Empire* (Chicago, 1973), 206-07.

<sup>27</sup> LaFeber, *The New Empire*, 141, *passim*. Herrick has American ships (and even Captain Togo in HJMS *Naniwa*) entering Pearl Harbor in the years before the War with Spain; *American Naval Revolution*, 105, 168, 199-200, 221. But the limiting depth at the mouth of the Pearl River was two fathoms, the House rejected an appropriation for dredging in 1897, funds did not become available until 1908, and the first major ship entered only in 1911. See Willis E. Snowbarger, "The Development of Pearl Harbor" (Ph.D. dissertation, University of California, Berkeley, 1950).



bases is to be taken as an index of outward-looking aspiration, it may be well to recognize that the real proliferation of overseas naval facilities came in the 1840s and 1850s, in the period of maritime greatness, when the United States had resident navy agents or naval storekeepers in London, Marseilles, Spezia, Porto Praya, Buenos Aires, St. Thomas, Rio de Janeiro, Lima, Valparaiso, Honolulu, Macao, and Shanghai.<sup>28</sup> Once again, perhaps, the wrong questions are at issue. Is not more to be learned about established national policy by asking not what proposals were made for the acquisition of coaling stations in the Caribbean or elsewhere, but rather why so little came of them? Why were the possibilities dangled at various times by Portuguese, Danes, Liberians, Peruvians, and Koreans not accepted? Why was so little done before the War with Spain to turn such available sites as Pearl Harbor and Pago Pago to strategic advantage? Why, indeed, was Pacific base development so slow after 1898?

Allied to the semantic question is the cartographic one: just as words are not things, a map is not the country it represents. In the late nineteenth century, the map was the same color from sea to shining sea, but the United States was, as it had always been, an Atlantic nation: on a Mercator world it belonged functionally at the left and not in the center. The frontier may have shaped the American character, but, whatever the history of the West Coast fur trade and of the California clippers, the United States remained an eastward-facing country. Despite the recorded aspirations of Pacific railway promoters and the later assertions of historians, the Far East was not a farther West. Aspiration was less substantial than geographic and economic reality.

For this assertion some evidence may be adduced. The *Empress of China* and a number of other American merchantmen had reached Canton by way of the Cape of Good Hope before Robert Gray arrived in the *Columbia* from the West Coast. American naval ships operated in the Indian Ocean long before the first one entered the Pacific. The sequence of commercial treaty negotiations with nonwestern societies proceeded eastward, from the Barbary powers to Turkey and thence to Muscat and Siam. Edmund Roberts went out by way of the Cape of Good Hope, and so did Caleb Cushing and Matthew C. Perry and Robert W. Shufeldt. The missionary effort indubitably had profound impact in Hawaii, but its first target had been India and its greatest efforts, until late in the century, were devoted to India and the Near East.<sup>29</sup> The big export-import trade was Atlantic in orientation, the product of the eastern concentration of population, wealth, and industry and of the receptivity of

<sup>28</sup> For navy agents, see the annual *Navy Register* for the 1840s and 1850s. Equally, one should not automatically equate the "New Navy" of the 1890s with a big navy: the total personnel strength of 1897 (11,985) was only 7 percent greater than that of 1845 (11,189), although the U.S. population had more than tripled. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* (Washington, 1960), 7, 736-37.

<sup>29</sup> James A. Field, Jr., "Near East Notes and Far East Queries," in John K. Fairbank, ed., *The Missionary Enterprise in China and America* (Cambridge, Mass., 1974), 25-26, 31-38; and Clifton J. Phillips, *America and the Pagan World* (Cambridge, Mass., 1969).



European markets to the torrent of agricultural exports that developed in the 1880s. Tenuously connected by rail with the eastern metropolis, the mountain and Pacific states contained in 1890 less than 5 percent of the nation's population. Of the 5 percent of total exports that emanated from the Pacific coast, a considerable portion consisted of grain bound eastward to Europe.<sup>30</sup> Strategically, the same was true: Europe, the traditional enemy, lay to the eastward, and European island bases watched the eastern seaboard; in the 1880s, with memories of the French in Mexico still green, the subdivision of Africa drew fresh attention to European capabilities, while the British occupation of Egypt and the presence of Ferdinand De Lesseps in Panama emphasized what attractive nuisances isthmuses could become. The existence of a perceived threat to the Atlantic coastline was evident in the Endicott Board's report on fortifications, the design and deployment of the New Navy, and the concern for Caribbean base facilities.<sup>31</sup>

In the Pacific and eastern Asia, by contrast, the position of the United States was marginal. Captain David Porter's early inspired admission of the inhabitants of Nukahiva to the "great American family" had been permitted to lapse, as had Perry's initiatives regarding Port Lloyd and Formosa. Against the annexation of archipelagos commenced by the French in 1842, the British in 1874, the Japanese in 1876, and the Germans in 1884, the United States could show only uninhabited and undredged Midway, some claims to guano islands, paper base rights at an unusable Pearl Harbor, and a share in the tripartite administration of Samoa. In China, it is true, the 1880s saw the missionary movement enter a period of remarkable growth, but in commercial matters the American position was weakening. China trade as a percentage of total foreign commerce had been declining since the 1840s, as trade with Europe grew and as Forbeses and Delanos, reacting to the lack of Asiatic investment opportunity, shifted their assets and energies from the treaty ports to western railroads. In commercial and political geography, moreover, the United States had suffered a considerable setback with the opening of the Suez Canal. Prior to 1869, when the route to the Far East was by way of the Cape of Good Hope, New York and Liverpool had been roughly equidistant from China, but now the Europeans were closer by the length of the Atlantic crossing. Although this situation provided southern textile exporters and their

<sup>30</sup> *Historical Statistics of the United States*, 7: 12, 537, 543; and John G. B. Hutchins, *The American Maritime Industries and Public Policy, 1789-1914* (Cambridge, Mass., 1941), 373-75. The government statistician Joseph Nimmo, Jr. commented in 1883 on the signal failure of the transcontinental railroad to generate Asiatic trade. Henry Nash Smith, *Virgin Land: The American West as Symbol and Myth* (Cambridge, Mass., 1950), 187.

<sup>31</sup> Davis, *Navy Second to None*, 23-31; Russell Weigley, *History of the United States Army* (New York, 1967), 284; and Walter Millis, *Arms and Men* (New York, 1956), 151, 159, 167. For concern about European economic and political ambitions in the Western Hemisphere, see David M. Pletcher, *The Awkward Years: American Foreign Relations under Garfield and Arthur* (Columbia, Mo., 1962), 129-34, *passim*. As early as 1880 Mahan had predicted that the prospect of an isthmian canal would require "a very large Navy . . . [or] we may as well shut up about the Monroe doctrine at once"; *Letters and Papers*, 1: 482. Ten years later he pinned his hopes for "the motive, if any there be, which will give the United States a navy" upon the isthmus; *Influence of Sea Power*, 88.

representatives in Congress with strong arguments for an isthmian canal, such a facility would be long in coming. For the balance of the nineteenth century European pressures on the Far East grew steadily, but until the Battle of Manila Bay American pressures did not.<sup>32</sup>

THE ATLANTIC ORIENTATION OF THE UNITED STATES was quite naturally reflected in the distribution of American-owned merchant shipping, whether of domestic or foreign registry. Of the former there was of course not much, and the postwar failure to restore the country's maritime greatness went far to justify Mahan's pessimism about "The United States Looking Outward," and his original choice of title for his article of 1890, "The United States Asleep." In the Atlantic, nevertheless, efforts at subsidized shipping lines had brought modest results in the establishment of continuous scheduled services with gulf and Caribbean ports and intermittent service with Brazil. "White-washed" iron and steel steamship tonnage—American-owned but under foreign registry—was also concentrated in the Atlantic. On the West Coast success was more limited. There, in 1865, the Pacific Mail Steamship Line had been granted a subsidy for monthly service to the Far East. The company's ships, which originally sailed via Hawaii, were soon shifted to the shorter and more economical northern route, with the result that in 1867 the North Pacific Transportation Company received a subsidy for a one-ship service between San Francisco and Honolulu. Such exiguous connections with the islands and the Orient seemed all the traffic would justify. Both lines found greater profit and employed more ships on coastal runs between Panama, Mazatlan, San Francisco, and Puget Sound.<sup>33</sup>

If, indeed, an expanding merchant marine is seen as an index of imperialistic tendencies, these were far less evident in the United States than elsewhere in the Pacific basin. In the Hawaiian Kingdom, of such small interest to American shipping interests, a period of insular expansionism brought the founding in 1881 of J. D. Spreckels's Oceanic Line, connecting Honolulu with San Francisco and reaching southwest to the Antipodes. The completion of the Canadian Pacific Railroad in 1885 was quickly followed by the establishment of subsidized steamship service between Vancouver and Hong Kong. But the truly important developments in Pacific shipping were those of the "insular imperialists" of Japan. In 1890 the 94,000-ton total of Japanese steam

<sup>32</sup> Field, "Near East Notes," 34-38; *Historical Statistics of the United States*, 550-53; Arthur M. Johnson and Barry E. Supple, *Boston Capitalists and Western Railroads* (Cambridge, Mass., 1967); Tyler Dennett, *Americans in Eastern Asia* (New York, 1922), 579-82, 586; and Akira Iriye, *Across the Pacific* (New York, 1967), 71-76, 80-81.

<sup>33</sup> Mahan, *Letters and Papers*, 2:28-29; Dennett, *Americans in Eastern Asia*, 584-85; and Hutchins, *Maritime Industries*, 424, 437, 482-516, 528-35. The superior attractions of the North Atlantic were strikingly demonstrated, even after the War with Spain had brought presumed Pacific opportunity, in the creation by J. P. Morgan of the International Mercantile Marine Co., the world's largest privately owned fleet, which in 1902 controlled 136 ships totalling more than a million gross tons, all in the Atlantic; Hutchins, *Maritime Industries*, 538-39.

tonnage was less than half that which the United States employed in international trade, but with the commencement of state subsidies in that year the Japanese merchant marine entered a period of explosive growth. By 1896 service had been established with China, the East Indies, Australia, San Francisco, and Seattle, and by 1900 the 543,000 tons of Japanese-flag steam tonnage totalled more than half again that of the United States. The contrast between this achievement and the difficulties that beset the McKinley administration in finding ships to carry troops to the Philippines (as indeed the contrast in end-of-the-century transpacific population movements) may serve as an indication of which Pacific power was outward-looking.<sup>34</sup>

The United States Navy in the 1890s was equally an East Coast Atlantic-oriented institution. In the outward-looking antebellum days, when American merchant shipping covered the globe, the navy had maintained six overseas cruising stations—Mediterranean, Pacific, West Indies, South Atlantic, East Indies, and African—to which the bulk of its active units was assigned. With the end of the Civil War this traditional practice was resumed, “irreflectively,” as Mahan later wrote, and by 1872 the Asiatic Squadron, successor to the old East Indies Squadron, had been brought up to a strength of eleven ships, more than a quarter of the total deployed on distant stations.<sup>35</sup> But this return to the ancient ways—also reflected in the concern of Secretary of State William M. Evarts and Secretary of the Navy Richard W. Thompson for foreign markets, in naval interest in the Amazon and Congo basins, Zanzibar, and Korea, and in Secretary of the Navy William E. Chandler’s call of 1883 for coaling stations everywhere—ended with the 1880s. The last years of the century witnessed an initial compartmentalization of the world in terms of naval power as the rivalries of the industrialized nations, the range limitations that accompanied the shift to steam, and the logistical and material requirements of modern warships brought a retirement of the world’s navies upon their home bases.

This development, most strikingly epitomized in the Royal Navy’s redeployment of 1904–05, had become apparent in the distribution of the United States fleet well before the War with Spain. With the shift to coal and the focus on the European threat, talk of markets and bases gave way in the reports of the secretaries to emphasis on foreign building programs and concern for the safety of the hemisphere. Since the coming of steam had given a new predictability to the movements of ships and squadrons, there also followed the first efforts at rational war planning. At the infant Naval War College the focus was on “what is necessary for a state of war,” and early

<sup>34</sup> *Cambridge History of the British Empire*, 3 (Cambridge, 1959): 200; Hutchins, *Maritime Industries*, 504, 515, 575; *Report of the Admiral of Navy, 1887*; Charles C. McLeod and Adam W. Kirkaldy, *The Trade, Commerce, and Shipping of the Empire* (New York, 1924), app. E; and Hilary Conroy, *The Japanese Frontier in Hawaii, 1868–1898* (Berkeley and Los Angeles, 1953).

<sup>35</sup> Alfred Thayer Mahan, *From Sail to Steam* (Boston, 1907), 196; and Charles C. Chadbourn III, “Sailors and Diplomats: U.S. Naval Operations in China, 1865–1877” (Ph.D. dissertation, University of Washington, 1976).

sessions concentrated on the study of Atlantic trade routes in preparation for efficient commerce raiding. In 1890 Captain Mahan was called to Washington to draw up contingency plans for hostilities with Great Britain or Spain; the possibility of a German takeover of Dutch possessions in the Western Hemisphere also excited concern. Starting in 1894, as the new battleships began to come into commission, the war college's annual problems focused on the defense of the Atlantic Coast, its lectures and war games emphasized the strategic geography of gulf and Caribbean, and its studies of international law dealt with such matters as the defense of the Hawaiian Kingdom against foreign aggression and the maintenance of the neutrality of an isthmian canal.<sup>36</sup>

Together with the new technology, these concerns governed the deployment of the New Navy. Despite the continuing problems of the China coast, the west coast of Latin America, the Bering Sea seal fisheries, Samoa, and Hawaii, the new steel ships were assigned in the first instance to the North Atlantic Squadron. The first modern warship to serve on the West Coast, the small cruiser *Charleston*, joined the Pacific Squadron only in 1890, and the show of force that followed the Chilean crisis was provided by units drawn from the Atlantic. Not until 1892, by which time eight protected cruisers had been commissioned, did the first new ship, the 900-ton gunboat *Petrel*, join the Asiatic Squadron. By 1897 the North Atlantic Squadron contained by far the largest concentration of available force. Compared with four first- and second-rates in the eastern Pacific and two in the Asiatic Squadron, the North Atlantic had ten; and, since these included five of the six battleships and both armored cruisers, the disproportion in fighting strength was far greater than the mere number of ships suggests.<sup>37</sup>

The limited forces assigned to the Pacific and Asiatic Squadrons hardly seem indicative of a forward policy. Such a policy, in any event, would not have been easy. The coming of steam had made the geography of coal and of coaling stations a question of prime strategic interest. The developed coal mines of the United States were in the East, and hopes of Alaskan coal deposits to ease the West Coast naval problem had not been fulfilled. Indeed, throughout the Pacific basin the supply of coal was effectively a British monopoly. The needs of the Hawaiian islands had traditionally been filled by

<sup>36</sup> Arthur J. Marder, *The Anatomy of British Sea Power* (New York, 1940), 491–92. The *Reports of the Secretary of the Navy* in the years from 1877 to 1887 stressed trade expansion, overseas bases, and the problem of the merchant marine; after 1889 the emphasis shifted to the defense of coastal cities. Mahan, *Letters and Papers*, 1: 444; for his 1890 “Contingency Plan of Operations in Case of War with Great Britain” and his comments on a plan of 1895 (after the first battleships had been commissioned), see *ibid.*, 3: 559–76, 2: 425–28; and for his assessment of the German threat, see *ibid.*, 2: 27, 37–38, and *Interest of America in Sea Power*, 15, 294–95. On naval war planning in the 1890s, see Ronald Spector, *Professors of War: The Naval War College and the Development of the Naval Profession* (Newport, R.I., 1977), 88–96. For visual evidence of the central strategic concerns, see the painting by Rufus Zogbaum, which shows the war college class of 1894 gaming the defense of an East Coast harbor (apparently Narragansett Bay) before a large wall chart of the Gulf and Caribbean; *Harper's Weekly*, 39 (1895): 149.

<sup>37</sup> *Report of the Secretary of the Navy, 1890, 1892, 1897*; and Robert E. Johnson, *Thence Round Cape Horn* (Annapolis, Md., 1963), 142, 145–48.

sailing ship from England, and the important new regional sources, Vancouver Island and New South Wales, were also British. American base facilities to mitigate the magnificent distances of the Pacific were lacking, and few seemed to care. The capabilities of West Coast navy yards were limited; the Hawaiian request for annexation had been rejected; and Pearl Harbor and Pago Pago remained undeveloped. The Asiatic Squadron was, in fact, an eastward projection of an Atlantic-facing country, and one greatly attenuated by distance. For materiel it depended on the New York Navy Yard, and its units traditionally proceeded to their duty stations by way of the Mediterranean and Suez, a passage extraordinarily vulnerable to European interference. With the German seizure of Tsingtao in 1897, the Asiatic Squadron became the only naval force in the Far East without a local base of its own.<sup>38</sup> The consequences of being so far out at the end of the line can be seen in Dewey's coal and ammunition problems and in the fact that when war came he not only had to destroy a fleet but also had to capture a harbor. These problems had been visible long before the war; they were reflected in the limited nature of the force that was maintained in Asiatic waters for the protection of American interests. In 1898 the Asiatic Squadron was outgunned by British, Russian, German, and Japanese forces in the area. Against a dozen major Japanese units, the United States boasted two—hardly an armament with which to go adventuring so far from home.<sup>39</sup>

BUT WHERE DOES ALL THIS LEAVE US? If we rule out the conventional wisdom about Darwin, the psychic crisis of imperialism, the New Navy, and the Pacific highway to Asia, what remains? What was, in fact, the nature of American relations with the outer world between the end of Reconstruction and the War with Spain?

Generally, it may be said, these relations were far more individual than governmental. As the wounds of war healed, an extraordinarily energetic society deployed its representatives abroad in a wide variety of roles: explorers, tourists, art collectors, philanthropists, missionaries, synarchists, railroad promoters, and mining engineers. Although much of this activity was sufficiently traditional, its scale and impact were increasing as a result of the remarkable economic development of these years, itself evidenced externally in three important and interconnected ways: the growth of North Atlantic

<sup>38</sup> Hutchins, *Maritime Industries*, 372. For a description of the navy's first attempt to ship American coal to Honolulu, see *Report of the Secretary of the Navy, 1891, 1896*. As late as 1913 coal was still a problem in the Pacific; O. J. Clinard, *Japan's Influence on American Naval Power, 1897-1917* (Berkeley and Los Angeles, 1947), 110. On bases, see Herrick, *Naval Revolution*, 164; and Seward W. Livermore, "American Naval-Base Policy in the Far East, 1850-1914," *Pacific Historical Review*, 13 (1944): 113-35. Mahan's own career reflected this traditional eastward deployment: although he served in both the Asiatic and Pacific Squadrons, he seems never to have crossed the Pacific or visited the Hawaiian Islands.

<sup>39</sup> *Report of the Secretary of the Navy, 1897*; Dennett, *Americans in Eastern Asia*, 615; Marder, *Anatomy of British Sea Power*, 304n; *Jane's Fighting Ships, 1898* (London, 1898); and Herbert, "Plea for the Navy," 1-15.

trade and the progressive integration of the North Atlantic economic community; the spillover of American enterprise into Canada, Mexico, and the Caribbean; and the beginnings of direct investment abroad. From these developments, Asia (especially China) was largely excluded: except for the skills of synarchists—half-missionary and half-mercenary in motivation—and the export of Christian truth, it was primarily a source of imports, not a market, and only the missionary effort grew significantly in the years before 1898.<sup>40</sup>

But, it may be objected, there were all those naval and diplomatic incidents of the 1880s and 1890s. Did these not constitute the preseason warm-up for imperialism and the projection of national power? Here two points may be noted. First, most of these incidents, like the greater part of the naval modernization program, took place in the administrations of the anti-imperialistic Cleveland, the same president who resisted the treaty provision for base rights at Pearl Harbor, refused to participate in the Berlin agreement on the Congo, withdrew the Hawaiian treaty of annexation, and declined to intervene in China or in Turkey on behalf of the Armenians. The Harrison administration did, it is true, conclude the Samoan treaty of 1889, mount an unsuccessful search for Caribbean bases, and support Hawaiian efforts at annexation; but its liveliest accomplishment was to preside over the consequences of the riot in the True Blue Saloon, and few have suggested that the United States contemplated the annexation of Chile. By contrast, the first Cleveland administration handled the first years of the Samoan fuss and carried out the Panama landings of 1885; the second managed the Brazilian intervention, the Corinto business, and the Venezuela boundary affair. Second, since most American naval interventions occurred in the Western Hemisphere and involved a real or presumed European presence, it would seem reasonable to see them as reflecting the same Monroeist (and Mahanist) strategy that governed the design and deployment of the New Navy. The importance of the Brazil trade, which greatly exceeded that with China, may be conceded, but Valparaiso, Corinto, and Caracas were hardly regions of any very impressive “new empire” economic interest at the time.

All this having been said, it is nevertheless still possible to find a persuasive linkage among economic activity, accelerating diplomacy, and naval demonstrations. To do this one must move again beyond economics and ideology to the generally neglected area of technological capabilities and consider, along with the consequences of steam propulsion, the revolution in communications that occurred with the coming of ocean cables. As these progressively joined

<sup>40</sup> For useful information on overseas activities in the 1880s, see Milton Plesur, *America's Outward Thrust* (DeKalb, Ill., 1971). Also see Merle Curti and Kendall Birr, *Prelude to Point Four* (Madison, Wisc., 1954); and Mira Wilkins, *The Emergence of Multinational Enterprise* (Cambridge, Mass., 1970). For a valuable and much neglected regional treatment, see J. Fred Rippy, *Latin America and the Industrial Age* (New York, 1944). On synarchy, see John K. Fairbank, “Synarchy under the Treaties,” in his *Chinese Thought and Institutions* (Chicago, 1957), 204–31; and James A. Field, Jr., “Transnationalism and the New Tribe,” *International Organization*, 25 (1971): 353–72. On the growth of missions, see Field, “Near East Notes,” 34–38; and, on the diplomatic response to missionary expansion, see Marilyn Blatt Young, “American Expansionism, 1870–1900: The Far East,” in Barton J. Bernstein, ed., *Towards a New Past* (New York, 1969), 186, 190–91.



together the already existing regional telegraph nets, those in the Western world who possessed the requisite skills and inclinations found it possible to administer modernity, public or private, at previously unheard-of distances. Without this development, it seems proper to suggest, the world would not have witnessed the late nineteenth-century growth of multinational enterprise, the particular kind of reactive and competitive European imperialism that marked the 1880s and 1890s, or the rash of international crises of the closing years of the century.<sup>41</sup>

Despite the traditional prominence of the name of Cyrus Field, this wiring of the world was, like so much of nineteenth-century history, primarily a British accomplishment. The principal inventions were British, gutta percha for cable insulation was a British imperial monopoly, in techniques of cable manufacture the British were far in the lead, and only in Britain was investment capital available on the required scale. Logically enough, then, the progress of long-range communications reflected the image of the world held by those looking outward from London. The Dover-Calais cable of 1851 was soon followed by other links to Europe. The Crimean War brought the extension of the overland telegraph, first to Balaclava and then to Constantinople. In 1865 Bombay and the Indian telegraph system was connected with that of Europe. In 1866 the Atlantic cable joined the European and North American telegraph nets. In 1870 an all-British cable route to India was opened, and in the next year Japan and the China coast were tied in, both by landline across Siberia and by cable via India, Singapore, and Saigon.

By the early 1870s, then, much of the world had been linked. When everything was working well, a telegram could be sent (albeit by way of London and at great expense) from San Francisco to Tokyo. But there were as yet no connections with interior China, with Africa south of Egypt, or with Latin America south of the Caribbean; and no cable yet spanned the Pacific. Not unnaturally, given their predominance in communications technology and international trade, it was the British who took the first great steps to link up the southern continents, with cables from Portugal to Brazil in 1874, and down the East African coast to Durban in 1879.<sup>42</sup> But, while the British were progressively tying together what seemed important to them, one American looking outward from New York was connecting up the Western Hemisphere. So now, if we need an "imperialist," or at least an individual who worked purposefully and effectively with Wall Street backing to increase his countrymen's capabilities abroad, we can have one. He is curiously absent from books on the "new empire," but he did exist. His name was James A. Scrymser.

Scrymser, in 1865, organized the International Ocean Telegraph Company. Two years later, armed with franchises from Congress and Spain and with

<sup>41</sup> For the history of ocean cables, see Charles Bright, *Submarine Telegraphs: Their History, Construction, and Working* (London, 1898); F. J. Brown, *The Cable and Wireless Communications of the World* (London, 1927); G. R. M. Garratt, *One Hundred Years of Submarine Cables* (London, 1950); George A. Schreiner, *Cables and Wireless and Their Role in the Foreign Relations of the United States* (Boston, 1924); and Leslie B. Tribolet, *The International Aspects of Electrical Communications in the Pacific Area* (Baltimore, 1929).

<sup>42</sup> Bright, *Submarine Telegraphs*; Brown, *Cable Communications*, 3–23; and Garratt, *Submarine Cables*, 2–30.

support from Secretary of State William H. Seward, he entered the Caribbean with a line from Florida to Havana. But further accomplishments in this area were frustrated by British competition and corporate infighting and there followed a change in aim. In 1878–79 Scrymser established, with financial backing from J. P. Morgan and other leaders of the New York financial community, the Mexican Telegraph Company and the Central and South American Telegraph Company and directed his efforts southward toward the west coast of South America. Mexico City was linked with Galveston in March 1881; by October 1882, the line had reached Lima with stops at various Central American way stations; in 1883 a Brazilian connection was established; in 1890 the cable was extended to Valparaiso, and in the next year the purchase of a trans-Andean telegraph line gave access to Argentina. The result of Scrymser's work was to make possible rapid communication between the United States and South America without having to route messages through London and Lisbon and with consequent greater speed, greater security, and diminished cost.<sup>43</sup>

Since the British were a nation of shopkeepers and the business of America was business, the purpose of the international cable net was to facilitate the work of the world and its geography reflected existing economic interest. But, as duplicate cables were laid on major routes, word-saving business codes were developed, and costs went down, the new technology took on its own creative role. With their lines eastward to Hong Kong and Shanghai, and across the South Atlantic to Brazil, the British enhanced their regional economic positions. In the Western Hemisphere the pattern of the American economic spillover largely replicated that of the telecommunications network. Increasingly, the successors of William Wheelwright and Henry Meiggs found themselves under the control of the home office—as was soon shown by the transfer of control of the Grace enterprises from Lima to New York in the 1880s and the appearance of two Grace brothers as members of Scrymser's board of directors. In the transatlantic context, where earlier attempts by American manufacturers to establish plants in Great Britain had failed, the years after 1866 brought routine success; as London became more and more the hub of world cable communications, firms such as Singer and Eastman Kodak increasingly entrusted the administration of Eastern Hemisphere business to their British branches.<sup>44</sup>

<sup>43</sup> *Personal Reminiscences of James A. Scrymser in Times of Peace and War* (n.p., 1915), 67–82; All-America Cables, Inc., *A Half-Century of Cable Service to the Three Americas, 1878–1928* (New York, 1928), 13–23; *New York Times*, Sept. 21 and 27, 1883; *National Cyclopaedia of American Biography*, 18 (New York, 1891): 314–15; and *Dictionary of American Biography*, 16: 521. The attractiveness of Scrymser's venture, as compared to contemporary Chinese promotional schemes, may be seen in the ease with which he gained financial backing and in his weighty board of directors, which included, in addition to J. P. Morgan, such worthies as John E. Alexandre (steamships), William R. and Michael P. Grace (South American trade and finance), Henry L. Higginson (banking), Charles Lanier (banking, railroads), and Richard W. Thompson (secretary of the navy, French Panama Canal Company).

<sup>44</sup> David Kahn, *The Codebreakers: The Story of Secret Writing* (New York, 1967), 835–45; All-America Cables, *Half-Century of Cable Service*, 96, 102; and Wilkins, *Emergence of Multinational Enterprise*, 30, 41, 44, 61, 114–15, 175–76.

In the Pacific the story was very different: there another history of non-accomplishment further emphasized the marginal nature of American interest. The Collins overland telegraph project had been killed off by the Atlantic cable. In 1870 Admiral David D. Porter urged a Pacific cable, a recommendation repeated by the Congress in 1873 and subsequently by Presidents Grant, Hayes, Cleveland, and Harrison, but to no avail. In Australia interest began to be evident in 1877 and in Canada soon after; in 1894 British imperial concern brought an abortive attempt to annex Necker Island in the Hawaiian chain as a relay station for a cable between Vancouver and the Antipodes. But, despite these signs of British interest and though the Hawaiians granted franchises in 1891 and 1895, though the navy made a hydrographic survey for a Hawaiian cable in 1891–92, though the Hawaiian commissioners urged action in 1893, and though Scrymser organized a Pacific Cable Company in 1896, nothing was done. By 1898 there were twelve active North Atlantic cables, nine of them duplex, and Scrymser had doubled up his South American lines five years before, but no action had yet been taken to span the Pacific.<sup>45</sup>

Although economic interests had shaped the ocean cable network, the blessings of rapid communications transcended the economic sphere. Capabilities are often as determining as intentions—as Mahan once observed, “‘Can,’ as well as ‘will,’ plays a large part in the decisions of life”—and the capability that now existed could be employed in various ways. The new speed of communication, combined with technological advances in printing and papermaking, gave rise to a revolution in journalism and ushered in the great age of the war correspondent, a profession in which such Americans as Januarius Aloysius MacGahan and Richard Harding Davis early attained high place. In countries afflicted with high literacy rates there developed an attentive audience for latter nineteenth-century theatrics—the Crimea, the Nile quest, the Bulgarian Horrors, the Armenian massacres, the sinking of the *Maine*—whose response in times of crisis constituted a new burden for their governments. Compared to the consequences of modern technology and universal education, the contributions of Charles Darwin to jingoism appear small.<sup>46</sup>

There were also more direct consequences for the foreign offices of the world. When information previously transmitted in intermittent chunks could move in a steady flow, and when dispatches that once took weeks or months in transit could now arrive in hours, a wholly new tempo of diplomatic activity

<sup>45</sup> Tribolet, *Electrical Communications*, 157–79; *Cambridge History of the British Empire*, 3: 411, 473–74; R. S. Kuykendall, *The Hawaiian Kingdom, 1874–93* (Honolulu, 1967), *passim*; Russ, *Hawaiian Republic*, 248–79; Garratt, *Submarine Cables*, 31; All-America Cables, *Half-Century of Cable Service*, 24.

<sup>46</sup> Mahan, *Interest of America in Sea Power*, 160; John W. Oliver, *History of American Technology* (New York, 1936), 445–48; and F. L. Bullard, *Famous War Correspondents* (Boston, 1914). This “new journalism” was but the latest (if also perhaps the greatest) of a series of advances in the influence of communications technology. In 1873 Congressman James A. Garfield had commented on the new impact of telegraph and press on the daily formulation of “all-pervading” public opinion; Leonard D. White, *The Republicans* (New York, 1938), 15. Similar developments had taken place in the years before the Mexican War; Merk, *Manifest Destiny*, 56–57.

developed. The apparent virtue of the quick response, as a means of increasing pressure on the French to withdraw from Mexico, led Seward to run up an enormous cable bill. In the *Virginus* affair of 1873, Scrymser's line to Havana and the pre-existing connections by way of London with Madrid and Mediterranean ports gave Hamilton Fish the country's first experience in "crisis management" and permitted the speedy recall of the European Squadron. From this time on the lives of secretaries of state would never be quite the same, and March 7, 1867, the date of appointment of the department's first telegrapher, may perhaps be taken as the date of origin of the "new paradigm" of diplomacy that developed as the century wore on.<sup>47</sup>

A final important consequence of the extension of ocean cables was the increased speed of naval movements. The revolution in propulsion that had imposed upon navies new base requirements and new limitations of range had also enabled them to steam upwind and in calm. Now, as consuls and ministers could cable for help and governments could cable orders, a wholly new speed of response developed. In 1861 the European Squadron did not reach New York until two and a half months after the firing on Fort Sumter. When summoned back in 1873 at the time of the *Virginus* crisis, it was home in five weeks; and from this time to the Boxer Rebellion a series of unprecedentedly quick arrivals of outside military forces hastened the course of history. The strategic significance of the submarine telegraph was early appreciated: by the late 1870s the Admiralty had cable communication with all important British overseas naval stations; in 1890, in his contingency plan for war with Britain, Captain Mahan called for an immediate cutting of the Halifax-Bermuda cable.<sup>48</sup> The importance of Scrymser's contribution to American capabilities in these matters can be seen in a comparison of the dilatory course of surface-mail diplomacy and the freedom of local initiative that marked the Samoan and Hawaiian questions with the rapid reaction to events in Panama, Chile, Brazil, and Nicaragua and the degree of central control maintained over these demonstrations.<sup>49</sup>

<sup>47</sup> Compared to same or next-day delivery by cable, late-nineteenth-century surface mail transit time for State Department dispatches was on the order of ten days to three weeks for Western Europe, three to four weeks for Russia, a month to six weeks for Argentina, Brazil, and Chile, and upwards of six weeks for East Asia. Department of State, *Instructions and Despatches*, *passim*. Seward's dispatch of November 23, 1866, which cost \$13,000 and took two days to decipher, was the wonder of the Parisian diplomatic corps. Beckles Willson, *American Ambassadors to France, 1777-1927* (London, 1928), 287; and John Bigelow, *Retrospections of an Active Life*, 3 (New York, 1909): 609-15, 622. Fish had made brief use of the cable in the Cuban incident of 1869; in the *Virginus* affair its employment was central. French E. Chadwick, *The Relations of the United States and Spain: Diplomacy* (New York, 1909), 294-97, 317-48; Graham H. Stuart, *The Department of State* (New York, 1949), 145; and Robert L. Beisner, *From the Old Diplomacy to the New, 1865-1900* (New York, 1975).

<sup>48</sup> Field, *Mediterranean World*, 305, 333-34; and Arthur Hezlet, *The Electron and Seapower* (London, 1975), 9. The first instance of continuous governmental control of a distant operation was the bombardment of Alexandria in 1882, when a British cable ship grappled the line from Malta and set up in business offshore. See Hezlet, *Electron and Seapower*, 18-19; Bright, *Submarine Telegraphs*, 174; and Mahan, *Letters and Papers*, 3: 562. Even more than the fuel question, the problems of communications technology tend to be slighted by naval historians: the anatomists of sea power concentrate on bone and muscle to the neglect of the alimentary and nervous systems.

<sup>49</sup> Scrymser quoted appreciative letters from naval officers for services during the Valparaiso crisis; *Pacific Cable: Private vs. Government Ownership* (New York, 1900), 9-10. But if his cable facilitated the solution, his company was part of the problem. The company called for naval protection during the Guatemala troubles

The responses of the United States government to these events in Latin America were, one may say, defensive in nature, comporting with the fears expressed by Secretary of the Navy Tracy of "the aggressive policy of foreign nations" and of the political threat posed by "the establishment of complete commercial supremacy by a European power in any state in the Western Hemisphere." They were part of intended American policy in a way in which the island conquests of 1898 were not. But when events in Cuba wrenched American policy into a new path, the new technology proved of governing importance in the conduct of what an accomplished army officer subsequently described as a war of "coal and cables." Starting in January 1898, cabled despatches were employed to accomplish a preliminary redeployment of the navy. Cable connections with the Far East (by way of London) carried the orders to Dewey to concentrate his force, keep full of coal, and proceed against the Philippines. A new cable from Hong Kong to Manila forewarned Admiral Montojo and permitted him to gather his ships in shoal water, but no cable yet reached Guam, where the garrison was taken by surprise.<sup>50</sup>

Energetically carrying out his orders, Dewey dispensed with the Spanish squadron and captured the harbor he had to have. In the American press the cabled reports of the battle produced a victory so famous that the politicians could hardly disown it, while the fact, made possible by these same cables' annihilation of distance, that Manila Bay preceded the Cuban landings by six weeks and the Battle of Santiago by two months, worked powerfully to focus public attention on the Far East. Now, at last, an "avalanche" of speeches, editorials, articles, and books did descend upon the American people. "Imperialism," we may say, was the product of Dewey's victory.<sup>51</sup>

It should be emphasized, however, that results had little to do with intentions. Faced with the problem of how to employ the Asiatic Squadron in the event of war with Spain, the naval planners had given no automatic priority to the Philippines. Among the possibilities considered in the year before the outbreak of hostilities was the withdrawal of these units westward to join in an attack on the Canaries. Even after Manila became the settled

of 1885 and exacerbated Chilean hostility in 1891. Scrymser, *Personal Reminiscences*, 83-84; Kenneth J. Hagan, *American Gunboat Diplomacy and the Old Navy, 1877-1889* (Westport, Conn., 1973), 171, 175; and *Report of the Secretary of the Navy, 1891*. Lord Salisbury once suggested that Britain, Germany, and the United States might find it worthwhile to guarantee the interest on a cable to Samoa so as to keep that situation under tighter control. G. H. Ryden, *The Foreign Policy of the United States in Relation to Samoa* (New Haven, 1933), 448. It seems possible that Frelinghuysen's ignorance of the capabilities of the world cable and telegraph network contributed to the failure of the 1882 Trescott mission to Chile. Charles S. Campbell, *The Transformation of American Foreign Relations, 1865-1900* (New York, 1976), 97-98; and Pletcher, *Awkward Years*, 91-92.

<sup>50</sup> *Report of the Secretary of the Navy, 1891, 1892*; George O. Squier, "The Influence of Submarine Cables upon Military and Naval Supremacy," *U.S. Naval Institute Proceedings*, 26 (1900): 599; French E. Chadwick, *The Relations of the United States and Spain: The Spanish-American War*, 2 vols. (New York, 1911), 1: 3-5; and Tribolet, *Electrical Communications*, 155, 245.

<sup>51</sup> Richard W. Leopold, *The Growth of American Foreign Policy* (New York, 1962), 150-52, 180-83; and Walter Millis, *The Martial Spirit* (Boston, 1931), 174-77, 195-99. Since naval officers are often grouped with the "imperialists," it may be worth noting that Dewey never recommended retention of the Philippines. A practical man, he wondered why, if colonies were indeed desired, the United States did not look to such handy locations as Mexico or Central America. Ronald Spector, *Admiral of the New Empire* (Baton Rouge, La., 1974), 83-91, 98-99; and Richard S. West, Jr., *Admirals of American Empire* (Indianapolis, 1948), 276.



objective, the aim was merely to gain leverage to pry Spain out of Cuba and to acquire security for the payment of an indemnity. The role reversal that found the Spanish giving up the islands and the United States doing the paying had occurred to none: in the words of the distinguished naval officer who wrote the history of the war, "Perhaps none were more surprised to find a great archipelago at their command than were the gentlemen composing the administration in Washington."<sup>52</sup>

Lamenting the "Great Aberration," Samuel Flagg Bemis once echoed McKinley's observation that, if only "old Dewey" had sailed along home after the battle, it would have saved much subsequent complication. It is in many ways an appealing thought, but to have done so would have been to abandon a war that had barely begun and to forego that "inducement" to Spain to leave Cuba that had been the object of the move against the Philippines. Yet, while departure was unthinkable, to remain was impossible without support from home. As Dewey swung around the anchor in Manila Bay, his ammunition depleted, his communications dependent upon British goodwill, and with an hourly diminishing coal supply, McKinley ordered forth a collier, a cruiser, two monitors, and some army troops.<sup>53</sup> This effort at reinforcement underlined once again the marginal American position in the Pacific. The eastward transfer of army forces for the campaign in Cuba had reduced the strength of the Department of California to 25 officers and 418 men and had stripped the Pacific Coast of arms and ammunition. The *Oregon* had been sent east and the cruiser *Charleston*, laid up at Mare Island, had yet to be reactivated. To obtain the twenty merchant ships required for the summer's troop movements, it proved necessary to threaten seizure of some and to transfer others from the Atlantic and from foreign flags. The logistics of the transpacific effort depended upon the enthusiastic unneutrality of the Hawaiian Republic.<sup>54</sup> But,

<sup>52</sup> Spector, *Admiral of the New Empire*, 32–36, and *Professors of War*, 89–94; Grenville and Young, *Politics, Strategy, and Diplomacy*, 267–96; H. G. Rickover, *How the Battleship Maine was Destroyed* (Washington, 1976), 10–13; Chadwick, *Spanish-American War*, 1: 90–91, 154, 208, 2: 472–73; and Dennett, *Americans in Eastern Asia*, 615–16, 618. Mahan later wrote that the aim of the war had been "to enforce the departure" of Spain from Cuba, commented on the surprising fact that trouble in Cuba had led onward to Asia, noted that the expansionist vision had never reached beyond Hawaii, observed that the Philippines had never previously risen above his "mental horizon," and attributed the outcome to the will of God. "The War on the Sea and Its Lessons, I: How the Motive of the War Gave Direction to its Earlier Movements," *McClure's Magazine*, 12 (1898): 110–18, *Problem of Asia*, 7, 11, and *Letters and Papers*, 2: 566, 579–80, 619. To the financial writer Charles A. Conant it seemed that traditional issues had suddenly been "tinged with a strange, new light by the flash of Dewey's guns in the Bay of Manila"; *The United States in the Orient* (Boston, 1900), 226.

<sup>53</sup> Bemis, *A Diplomatic History of the United States* (New York, 1936), 467. Dewey's victory gave rise to novel and difficult problems in the areas of coal and communications. Since the Spanish refused him use of the Manila-Hong Kong cable, his telegraphic dispatches to Washington had first to travel by ship to Hong Kong. The need to fuel *McCulloch* for the round trip (since coal for warlike use was contraband and only on condition of immediately heading homeward could any of his ships have coaled in neutral ports) was in part responsible for the delayed transmission of his action report. Prior to 1898 the British had worried about the vulnerability of their worldwide cable system; since transmission by a neutral of belligerent dispatches raised serious problems of international law, the Americans (and doubtless others) had worried about British control of the world cable network. Learning of Dewey's isolation, Scrymser at once offered to lay a new cable between Hong Kong and Manila with financing by J. P. Morgan, but the proposal was refused by London. The British at Hong Kong did, however, accept Dewey's dispatches under the fiction that the traffic was nonmilitary; it is interesting to speculate on his position had they refused to bend the rules. The five-day minimum reply time imposed by the need to communicate by ship with the Asiatic mainland complicated dealings with Aguinaldo and von Diederichs and made the situation in Manila Bay



once the troops did reach the Philippines, they stayed; and the problems of administering so distant a dependency brought forth the single preplanned insular annexation of the period, as Wake Island was taken as a landing station for the now-imperative American Pacific cable.

RETURNING FINALLY TO THE "WORST CHAPTER," we may conclude that much of it is wrong and most of it irrelevant to "imperialism" and the events of 1898.<sup>55</sup> The New Navy was a defensive answer to European developments; its deployment reflected a shrunken rather than an enlarged strategic perimeter. The "search for bases" was a response to the strategic problems of isthmus, Caribbean, and eastern Pacific. Neither missionary work nor burgeoning exports nor the beginnings of foreign investment called for the extension of political control. The ideologists, so selectively quoted by posterity, were of negligible importance. What Americans, whether travellers or missionaries or businessmen, wanted of the outer world was the freedom to pursue happiness, to do their thing, to operate insofar as possible unhindered by arbitrary power or obsolete ideas.<sup>56</sup> Proud of their own self-determined independence, they

(as in Samoa and Hawaii) one where central control was diminished and events tended to take charge. Elbert J. Benton, *International Law and Diplomacy of the Spanish-American War* (Baltimore, 1909), 190-91, 212-13; Chadwick, *Spanish-American War*, 1: 212, 2: 363, 423, 425, 441; "Danger to Submarine Cables in Case of War," *Scientific American Supplement*, May 1, 1897, p. 17798; "The Neutral Use of Cables," *ibid.*, March 3, 1898, pp. 18489-90; Scrymser, *Pacific Cable*, 2-8, and *Personal Reminiscences*, 93-100; Department of State, Instructions, Great Britain, May 22, June 6, 1898, and Despatches, Great Britain, May 11, 24, June 1, 7, 1898; and William R. Braisted, *The United States Navy in the Pacific, 1897-1909* (Austin, Texas, 1958), 35, 43, 45-46.

<sup>54</sup> Chadwick, *Spanish-American War*, 2: 369; W. Cameron Forbes, *The Philippine Islands*, 1, (Boston, 1928): 69; U.S. Naval History Division, *Dictionary of American Naval Fighting Ships* (Washington, 1959-), 2: 82; James A. Huston, *The Sinews of War: Army Logistics, 1775-1953* (Washington, 1966), 286; and Clinard, *Japan's Influence*, 16-17.

<sup>55</sup> It sometimes seems as difficult to discover the "very heart of contemporary revisionism" as it is to elucidate the message of Mahan. If it is that after 1898 American policy makers sought to preserve a "long-term option" in China, one can hardly argue: that is what sensible policy makers try to do. But if, regarding the War with Spain, the "central question" turns on the contention that the acquisition of the Philippines was "the product of a conscious, pragmatic effort to provide . . . integrated, protectible trade routes across the Pacific," the policy seems irrational, the argument dubious, and the evidence lacking. Thomas J. McCormick, "American Expansion in China," *AHR*, 75 (1970): 1394-96. What is an "integrated" trade route? Why across the Pacific? The American import-export community was in the East, and the China trade was an Atlantic trade. The route westward from New York was shorter, and faster for passengers (like Commodore Dewey, going out to his new command) and mail. But for freight it involved 3,000 miles of high-cost railroad carriage followed by the transit of an ocean where shipping was scarce and coal much more expensive than on the Atlantic-Suez route. As to whether this route was "protectible," the question is, against whom? Nobody ever solved the problem of the defense of the Philippines. In the early years they seemed vulnerable to German attack; as late as 1911 Mahan conceded to Japan the capability of bagging the Philippines, Guam, and Hawaii and landing on the West Coast before the battle fleet could reach the scene of action. Clinard, *Japan's Influence*, 36-40; and Mahan, *Letters and Papers*, 3: 384-85. For an interesting visual aid, which may have encouraged latter-day geopolitical misconception, see the schematic map of "Geography and American Sea Power, 1898-1922," in Harold and Margaret Sprout, *Toward a New Order of Sea Power* (Princeton, 1940), 22 (reprinted in E. M. Earle, ed., *Makers of Modern Strategy* [Princeton, 1943], 428). "Sea Power" appears in the form of enormous arrows projecting from the East and West Coasts, from the isthmus, and from the Hawaiian Islands, with those in the Pacific much the largest. Yet for most of the period in question the entrance to Pearl Harbor was not dredged, the Panama Canal was not completed, and the battle fleet was in the Atlantic.

<sup>56</sup> On this same phenomenon in a more recent context, see Samuel P. Huntington, "Transnational Organizations in World Politics," *World Politics*, 25 (1972-73): 333-68.

were sympathetic to similar desires on the part of Samoan chiefs, Korean kings, Egyptian khedives, Armenian Christians, Brazilians, Venezuelans, and Chinese. Most of all, because they were nearest and most visible and noisiest, it was the Cubans who engaged this sympathy.

Such traditional aims and attitudes had little to do with any "new American spirit," or with Asiatic markets, or with "insular imperialism." The western Pacific acquisitions, which opened a new era of American history, were in one sense the product of the new technological developments; in another, they can be seen as historical "accidents." If the British had kept the Philippines after the Seven Years' War, the War with Spain would have been confined to the Atlantic. If the Filipinos had been happy under Spanish rule (or if the Asiatic Squadron had been sent against the Canaries), this rule might well have continued. If Spain had either pacified or given up Cuba, there would have been no war; perhaps, indeed, war might have been averted had the *Maine* not been sent to Havana.<sup>57</sup> If this was indeed the case (and unless a conspiracy theory is at once developed on the basis of Lodge's prediction of "an explosion any day in Cuba"), the identity of the ship sent is perhaps the ultimate historical accident. A sunken *Texas*, say, would have contributed little to the torchlight parades of chanting patriots.

If there had been no war, the process of the "Americanization of the world," which so commended itself to the British reformer and journalist W. T. Stead (and which so exasperated other Englishmen), would no doubt have continued.<sup>58</sup> American heiresses would have continued to marry British milords; missionaries and student volunteers would have persisted in their work for China; engineers, promoters, and salesmen would have pressed on with their activities; multinational corporations would have continued to expand; Carnegie libraries would have gone on proliferating throughout the English-speaking world. But in political terms the outward thrust would, in all probability, have conformed to the relaxed anticipations of "Jingo Jim" Blaine, the strategic parameters of the "imperialistic" Mahan, and the proposals of the Republican platform of 1896: a suitable measure of control over the impending isthmian canal, over protective Caribbean naval bases, and over the Hawaiian islands. In the circumstances of the time, such a defensive policy seems quite reasonable.

<sup>57</sup> "Apparently as the result of an accident in Havana Harbor, the path of destiny has been opened for us in the East"; Conant, *United States in the Orient*, 63.

<sup>58</sup> W. T. Stead, *The Americanization of the World; or, The Trend of the Twentieth Century* (New York, 1902); and R. H. Heindel, *The American Impact on Great Britain, 1898-1914* (Philadelphia, 1940), 138-70, *passim*.

## *Comments:*

THE 1890S—THE DECADE THAT INAUGURATED modern America and particularly modern American foreign policy—continues to fascinate many American historians. James A. Field, Jr., however, believes these years to be the “worst chapter” in American diplomatic historiography. He ascribes the problem in part to historians who cling to certain conventional wisdom which, upon examination, should no longer be either conventional or considered wise. The long misplaced emphasis on Social Darwinism is certainly an example of such mistaken conventional wisdom. Thomas McCormick, Paul Holbo, and, in unpublished work, Robert Dawidoff pointed out long ago that Social Darwinism was more a rationale for, than a cause of, American expansionism. Other historians have also previously argued Field’s point that, when Josiah Strong discussed foreign policy, he wanted to stress “what was at stake at home.” Indeed, these historians have argued that almost every American policy maker or public spokesman who discusses foreign policy emphasizes what is at stake at home.

Although the correction of such details is important, Field dismisses the more critical issue of how men and women of the 1890s conceptualized problems in foreign policy and developed (if they were lucky) solutions. In the process Field even attacks many recent historians who have tried to place the 1890s in this broader framework. In important respects, the first section of this essay is misleading, for the later emphasis on cable communication, while adding an important dimension to our knowledge of late nineteenth-century expansionism, builds on the work and the approaches of, among others, Ernest Paolino, Charles Vevier, Harold Schonberger, and David Pletcher. This is all to the good. The story of how this communication network developed complements much of what we already know and makes it that much easier for us to think in broad terms about the importance of the 1890s.

THE ESSAY, HOWEVER, ULTIMATELY FAILS to reconceptualize the foreign policies of the 1890s. One reason, the less important, is specific: at the critical point—1898—Field is forced to drop his thesis and instead emphasize “accidents” in history. This escape hatch is familiar. In Samuel Flagg Bemis’s seminal text on American diplomatic history, written forty years ago, the grand story of American expansion rolls along until the narrative encounters 1898. Bemis could handle the next thirteen years only by calling them an “aberration.” Such an explanation does not suffice, for it neither explains the particular events nor develops a framework that allows us to understand the era’s importance in the totality of American history. The “aberration” is not dealt with adequately because any attempt to claim that the explosion that

sank the *Maine* in February 1898 propelled the United States into war and that the explosion was one of those “accidents” of history begs the central question of why the *Maine* was in Havana harbor in the first place. The answer lies in President McKinley’s decision to send it there to protect American lives and property after riots in mid-January indicated that Spanish reforms were not cooling the Cuban revolution and that Spain was losing control. Sending the *Maine* can thus be understood as marking an important change in the president’s previous policy of watchful waiting, and it anticipates his demands in the final ultimatum that Spain accept an American presence to mediate an end to the revolution. The February explosion may have been accidental, not Spanish-inspired, but there was nothing accidental about the causes or the consequences of the *Maine*’s visit.

This particular problem is part of the larger problem, confronted by most historians of the era. How do the events before 1898 relate to events after 1898? Field sees little relationship. He argues, for example, that United States officials must not have been concerned about Asia before 1898 since so few warships were stationed in the Pacific. When they did consider the area, the officials viewed it as an eastern rather than a western problem. With few exceptions, however, the essay’s examples of Americans traveling east to reach Asia are chosen from the years before 1846, that is, before the United States controlled the West Coast. By the 1890s, Seward had talked of traveling to Asia via the Aleutians, Grover Cleveland had called Hawaii (not the Cape of Good Hope) the stepping-stone to Asia, and Theodore Roosevelt was about to proclaim the “Pacific era” of American history.

The question is not how many warships, or cables, were in the Pacific. Neither naval officers nor cable owners made basic foreign policy decisions between 1890 and 1901—or after. (Field notes that James A. Scrymser is “curiously absent from books on the ‘New Empire.’” There is nothing curious about it. A number of people are absent from my own account of the “new empire,” most of them more important than Scrymser. In weak moments I fear such people may even number in the tens.) The absence of warships did not mean that political figures and business groups were not interested in the Pacific. From the mid-1890s until at least 1906, Alfred Thayer Mahan, for example, increasingly viewed the Pacific as an arena of great-importance to the United States. He included Hawaii in this arena, since it is in the Pacific and several thousand miles from the West Coast. In his biography of Mahan, William E. Livezey has suggested that the officer’s “program of action” can be summarized as “dominance in the Caribbean, equality and cooperation in the Pacific,” and “interested abstention” from strictly European rivalries on the Continent. By 1910, however, Livezey has noted that Japan’s rise forced Mahan to plot a retreat from the Pacific.<sup>1</sup> Field emphasizes the later withdrawal but not the advance between the 1890s and 1906. Brooks Adams remains important for the same reasons. As Arthur F. Beringause has

<sup>1</sup> Livezey, *Mahan on Sea Power* (Norman, Okla., 1947), especially chap. 10.

pointed out, Adams, Roosevelt, and Henry Cabot Lodge met in Henry Adams's house to plot strategy in 1897, and by the end of that year they reached a consensus on policy. And Thomas J. McCormick has argued that Roosevelt carried that consensus to McKinley.<sup>2</sup>

To restate, the question is not how many battleships or cables were in the Pacific before 1898, but why McKinley and his advisers were able to move so rapidly into the Pacific after Dewey's victory at Manila Bay. Historians have now proven beyond doubt that the policy makers in 1898, like President Cleveland and Secretary of State Bayard before them, understood not only that American interests were developing in Asia but also that they were responsible for defending those interests. Such an argument hardly means that Latin America was unimportant; it was indeed primary in Washington's foreign policy priorities. Field's emphasis on South and Central America thus fits in well with the work of many historians who have noted the significance of the Venezuelan and Cuban crises during the 1890s. Those historians, however, have not argued that Washington officials were incapable of thinking about Latin America and Asia at the same time.

Since the term "new empire" did not appear until 1902, Field cannot understand how my own work has "persuaded many that the concept existed in its own right" before 1902. That puzzlement pinpoints the central problem in the essay. If, the essay asks, the cable system and battleship deployment indicated little interest in Pacific affairs before 1898, why did the United States control a "new empire" by 1900?

The result was no more accidental than McKinley sending the *Maine* to Havana. The course of an earlier empire provides a helpful analogy. By 450 B.C. the Athenians, who had once belonged to a "league," had slowly developed and expanded their own control until they transformed that alliance into their own empire. The result was not termed an empire until after 450 B.C., but the concept and the methods of empire preceded that date. As Russell Meiggs has written, even while Athens and its allies called their relationship a league or alliance, "the tools of empire had already been forged."<sup>3</sup> In the same respect, the term "new empire" was perhaps not known until 1902, but the tools of empire were forged throughout the 1890s. The cable system was one of those tools, but it was hardly the entire toolbox. Others have also identified those tools, and it is inexcusable to say of their work that "much of it is wrong and most of it irrelevant to 'imperialism' and the events of 1898," especially since Field builds on the work of these historians but cannot explain some of the events these historians have succeeded in explaining.

The essay helps to paint our composite picture of the 1890s, but it is unable to finish the job for in the end it cannot explain the policies of 1898 to 1901. It

<sup>2</sup> Beringause, *Brooks Adams: A Biography* (New York, 1955), 156-60; and McCormick, *China Market: America's Quest for Informal Empire, 1893-1901* (Chicago, 1967).

<sup>3</sup> Meiggs, *The Athenian Empire* (London, 1972), 153. I am indebted to Paul Rahe of Cornell University for this reference.

confuses cables with messages. An explanation of the foreign policies of the 1890s lies less with the communications network than with what officials were saying over the network, and this problem is not considered in the essay. In the 1890s, the media was not yet the message.

Field thus tells us why McKinley should not have annexed the Philippines and made the United States a major Pacific power, but to discover why the president did accomplish those objectives we must return to the work of McCormick, Charles S. Campbell, Jr., William Appleman Williams, Sylvester K. Stevens (who explains the importance if not “magnificence” of Pearl Harbor by the late 1880s), Paul Varg, Ernest May, H. Wayne Morgan, Marilyn Blatt Young, and others. For they have told us how the “tools of empire” that were forged in the 1890s constructed the new foreign policy edifice of 1898 and after. And few, if any of them, have to resort to Social Darwinism as an explanation. Whether or not we agree with their individual interpretations, their work forces us to conceptualize the era as a whole and to reconsider its roots and its significance for our time.

If the 1890s is the “worst chapter” in American diplomatic historiography, and I do not believe it is, the fault does not belong to those who have replaced “aberration” and “accident” with more useful, coherent, and defensible approaches to understanding the decade. Nor did those scholars believe it necessary to build an eye-catching structure on the historical landscape by attempting to reduce the already existing structures to rubble.

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JAMES A. FIELD, JR.'S EFFORT TO RESTRUCTURE the long-standing debate about “American imperialism” especially interests me because of my own recent attempt at the same task.<sup>1</sup> As we all know, scholarly disputes often end in impasse. One scholar argues that unicorns are handsome; another contends that they are ugly; their disciples elaborate the cases pro and con without, however, either changing the terms of argument or resolving the dispute. What we usually need at such times are new questions and an alteration in the terms of debate—perhaps suggested by someone who does not care

<sup>1</sup> Robert L. Beisner, *From the Old Diplomacy to the New, 1865–1900* (New York, 1975).



whether unicorns are handsome or ugly, but who is instead fascinated by their relationship to griffins. Some kind of radical perspective is essential to shoving an old debate in a new direction. Otherwise, the ancient dispute is likely to persist tediously, fueled by students trying to make their mark with a conspicuous entrance into the established debate, by scholars focusing on “false dichotomous questions,”<sup>2</sup> or by historians simply miring themselves in the deadening sludge of old questions.

Something of this sort has occurred in the study of late-nineteenth-century American imperialism. The debate about its chronology and its underlying impulses and motives has persisted at least since the publication of Julius W. Pratt’s *Expansionists of 1898* in 1936. It took on new but not *different* life with the publication of William A. Williams’s *The Tragedy of American Diplomacy* (1959) and Walter LaFeber’s *The New Empire: An Interpretation of American Expansion, 1860–1898* (1963). In all the years since Pratt’s work, historians have been wrangling over such false dichotomous questions as

1898: Aberration or Culmination?

The Spanish-American War: Popular Crusade or Drive for Markets?

McKinley: Chocolate Eclair or Clever Statesman?

Specifically, the debate, which actually encompasses some disparate and distinct interpretative problems, has been dominated by five main issues: (1) the “continuity” issue—that is, whether the events of the late 1890s represented an abrupt departure from the past or the logical product of earlier developments and trends; (2) the “realism” issue—that is, whether the United States sallied off to war in 1898 for emotional and idealistic reasons or fought to achieve “realistic” objectives; (3) the “economic” issue—that is, whether American imperialism was the result of a drive to expand foreign export markets or the consequence of ideological, security, and other motives; (4) the “semantic” issue—that is, whether the phenomena of the 1890s should be called “imperialism,” “expansionism,” or something else; and (5) the “deliberateness” issue—that is, whether the key events by which we define American imperialism were the result of bungling, emotionalism, and “accident” or the consequences of forethought and deliberate calculation.<sup>3</sup> The current state of the traditional debate is codified, so to speak, in Charles S. Campbell’s *The Transformation of American Foreign Relations, 1865–1900* (1976), which is a thorough and excellent book but one that in no way tries to transform the terms of debate.

CLEARLY, THE TIME HAS COME for a new departure in the study of American imperialism, and it is in this light that I have approached James Field’s essay, particularly since his title suggests that his purpose is to rewrite “The Worst Chapter in Almost Any Book.” Has he merely offered new fuel to the old

<sup>2</sup> David Hackett Fischer, *Historians’ Fallacies: Toward a Logic of Historical Thought* (New York, 1970), 9–12.

<sup>3</sup> Beisner, *From the Old Diplomacy to the New*, 11–33.

debate? Or has he redefined the debate itself? Has he produced a provocative new argument for the beauty of the unicorn? Or has he introduced us to a fabulous new field grazed also by griffins? Field states his first general criticisms of past interpretations with an élan that suggests we are to witness new ground being broken. I heartily concur that presentism, moralism, and a proclivity to see false continuities have had a “tendentious” impact on general views of U.S. foreign relations from 1865 to 1898, though I boggle at the casualness with which he suggests that historians, through an act of will, can avoid “seeing the past through the prism of the present . . . .” I also agree that most interpretations of American imperialism are too “rational,” too “unitary,” too “ethnocentric,” too oblivious of the “environment,” and too dependent on asking the wrong questions. Most of the critical parts of his essay—most notably his devastating assault on those attributing great influence to Social Darwinists and other intellectuals—are persuasive. But they do not take us anywhere new. His criticisms of past work in themselves neither define nor clearly point the way toward a novel conception of the historical problem of “American imperialism.”

Field’s contribution is not, then, his critique but his own new descriptive material. Briefly recapitulated, Field says that

1. The advent of steam propulsion bound modern navies to coal deposits and stations and impelled the United States Navy to abandon far-flung patrols for the security of its own home bases. Thus, the “New Navy” of the 1890s, however enlarged and modernized, was tied even closer to home and to defensive positions than it had been in the 1880s.
2. Instead of feeling a constant tug westward toward Asia, American exporters, shippers, diplomatic policy makers, and navalists were actually eastward-oriented throughout the era of the “new empire.” When Americans “looked outward” for personal contacts or for economic opportunities abroad and when they hunted for potential threats to national security, they looked across the Atlantic, as they always had.
3. The rapid development of a vast network of telegraphic cables had linked Washington and New York to the rest of the world by 1898 but not yet by a direct route across the Pacific to Asia. The cable network can be seen reflected in America’s Atlantic orientation and emphasis on hemispheric defense. The cables stimulated business activity abroad, accelerated the pace of diplomacy, shaped maritime strategy, and were eventually crucial in abruptly entangling the United States in the politics of East Asia in 1898.

Still, Field’s general purpose is not clear. Is this new material the foundation for a “best chapter” on American imperialism? It may be, but this is certainly not self-evident. Reduced to its essentials—however persuasive and however

fascinating—it does not *explain* the historical phenomenon of American imperialism. Instead, by marshalling mostly new evidence, Field has simply produced a set of unfamiliar inferences (descriptive statements) we can make about the past. Thus, just as his spirited destruction of the old saw that intellectuals somehow brought about American imperialism boils down in part to the descriptive statement that “intellectuals neither advocated imperialist policies nor had access to foreign policy makers as much as former historians have maintained,” so his new material finally comes down to the following historical inferences:

1. Naval developments in the 1890s betoken a defensive and hemispheric rather than an offensive and imperialist stance.
2. The United States looked eastward rather than westward, and its activities conformed with this point of view.
3. Although generally ignored by historians, the development of an international network of telegraphic cables, which did not include a link across the Pacific between the United States and East Asia, was quite important in this period.<sup>4</sup>

WHAT CAN BRING THESE NEW DESCRIPTIONS of the past together into a new explanation of American imperialism or, at least, an argument that would alter the old terms of debate? Though at times Field comes tantalizingly close, he nonetheless fails to propose a general theory that can connect his new knowledge about diplomacy with an explanation of why things happened as they did. Implicitly, at least, he seems to favor some kind of materialistic theory of history. Underlying all of his novel “theses” are the development of two new technologies—steam propulsion and international cables—and new patterns of maritime and diplomatic traffic. Some of his direct statements seem to echo a hidden theoretical commitment: other historians have “words do duty for things and presumed intentions for actual capabilities”; “just as words are not things, a map is not the country it represents”; “aspiration was less substantial than geographic and economic reality”; “capabilities are often as determining as intentions”; “the anatomists of sea power concentrate on bone and muscle to the neglect of the alimentary and nervous system”; and “results had little to do with intentions.” One should especially note his remark that we “must move . . . beyond economics and ideology to the . . . area of technological capabilities . . . .”

Is Field suggesting a materialist (or perhaps behaviorist or environmentalist) theory of history to explain American imperialism? Unfortunately, we do not know, because Field never closes with the issue. And that reluctance to come to grips with theory mars other parts of his essay, especially considering its ambitious objectives. In the absence of an explicit theory discounting the

<sup>4</sup>For an elaborate, yet clear, discussion of the distinction between historical inference and historical explanation, see Allan J. Lichtman and Valerie French, *Historians and the Living Past: The Theory and Practice of Historical Study* (Arlington Heights, Ill., 1978).

relevance of ideas, sentiments, and rhetoric, for instance, I find his treatment of them wholly unsatisfactory. I am not referring here to his contention that certain intellectuals either did not think what has been attributed to them or did not possess the influence attributed to them (a contention I believe Field convincingly supports). I have in mind, rather, his almost total neglect of ideas. Yes, it would certainly appear that the development of cables was quite important in the acceleration of diplomacy. But which men chose to use those cables, to what purpose, and why? Cables alone cannot dictate the goals of diplomacy any more than computers can determine the policies of industry. Certainly, the availability of a new technological capability might influence the decisions of those responsible for making policy. But how are we going to understand why they chose to do one thing rather than another with the technology unless we determine what they thought, believed, and were concerned about? The U.S. government could choose or not choose to build modern steam-propelled warships. It could send Dewey's squadron to East Asia or somewhere else. Technology cannot account for the decisions that were made, only that certain kinds of decisions could be made.

Field's analysis seems based on what Lionel Trilling has described as the "liberal" sense of reality, which is seen as "hard, resistant, unformed, impenetrable, and unpleasant."<sup>6</sup> Such a perspective caused historians like Charles A. Beard to assume that they could not "really" explain what motivated the Founding Fathers unless they could point to something concrete like land deeds or stock certificates. Given his criticism of the economic approach to American diplomatic history, one would think that Field might apply Trilling's observation to those who find export statistics at the bottom of American imperialism. Nonetheless, his own essay relies exclusively on such "hard" data as steam boilers, coal bins, shipping routes, and telegraphic cables. But we do not know whether he really believes in such a materialist sense of reality because Field has chosen not to grapple with theory directly. This becomes painfully obvious at the end of his essay when, alarmingly, he suddenly states that Americans wanted "the freedom to do their thing, to operate insofar as possible unhindered by arbitrary power or obsolete ideas," that they were "proud of their own self-determined independence," and "sympathetic to similar desires" elsewhere. What is this if not a purely intellectual explanation and, as well, unitary, ethnocentric, and undocumented?

This disinclination to deal directly with the theoretical bases of his arguments and assumptions also partially accounts for some of the highly dubious causal statements in the essay. Can historians of American imperialism rest content with the statement that Commodore Dewey "had to" destroy the Spanish fleet and seize Manila harbor as "a consequence" of "being so far out at the end of the [eastward-directed coal and ammunition] line"? Did new communications technology combined with literate publics lead to an "attentive audience" for war correspondents and thus to emotion-induced "times of

<sup>6</sup> Trilling, *The Liberal Imagination: Essays in Literature and Society* (New York, 1950), chap. 1.

crisis"? Why was the audience interested in war correspondents rather than architectural news? Was the U.S. government really so helplessly impressionable? " 'Imperialism,' we may say," Field concludes, "was the product of Dewey's victory." Even allowing for hyperbole or deliberate oversimplification, this statement represents a crude definition of imperialism, a nonchalant dismissal of abundant evidence suggesting other conclusions, as well as a simplistic notion of causality. Even worse are the "arguments" about the "accidental" character of American imperialism that riddle Field's next-to-last paragraph, arguments that do violence to the subtle philosophical and theoretical problems involved in the concept of "historical accident." One could equally well argue that the American Revolution was an accident in that it would not have occurred "if the [colonists] had been happy under [British] rule" or that there would have been no Cold War if Stalin had been converted to capitalism.

FIELD ASKS, "BUT WHERE DOES ALL THIS LEAVE US?" About where we were before, I'm afraid. Some of the old debaters should be out hunting for a white flag to wave, perhaps, and much of Field's new material is suggestive. But its importance depends on what is made of it. It might be possible to argue, for instance, that accelerated diplomacy, in part a product of telegraphic cables, in turn helped to persuade American policy makers in the 1890s that they were performing in a new and more dangerous era than before. The Atlantic-orientation thesis—which needs to be supplemented with social and cultural evidence to become fully convincing—does not necessarily entail Field's conclusion that the American drive toward Asia was weak. I would argue instead that America's preoccupation with Hawaii, China, the Philippines, and Korea, despite the nation's traditional and still-intact Atlantic orientation, graphically demonstrates the strength of the "new paradigm" in American diplomacy that arose in the 1890s.<sup>6</sup> Policy makers' decisions do not depend alone on the circumstances of the so-called real world (for example, cables and ships cross the Atlantic but not the Pacific) nor on what retrospective commentators think they should have done (for example, the general consensus today that the United States greatly exaggerated the importance of East Asia eighty years ago). They also depend on the lenses through which those policy makers "see" the real world, lenses tinted by altering circumstances and shifting perspectives. And the evidence remains convincing that American statesmen at the end of the nineteenth century believed profoundly that the international stakes in Asia were high. It was to Asia, not to Europe, that the United States sent warships and occupation troops; it was in Asia where the United States adamantly retained control of territory, even in the face of armed insurrection; and it was in Asia where the United States joined other great powers in disciplining the Chinese Boxers. How extraordinary,

<sup>6</sup> Beisner, *From the Old Diplomacy to the New*, 82–83, 85, 134–35.

considering that the cables, dollars, and freighters were all spanning the Atlantic instead of the Pacific!

Thus, Field's essay has neither given us a persuasive new explanation of "American imperialism" nor substantially altered the terms of the now-wearisome debate. Although he is clearly uncomfortable with the present terms of debate, he does not manage to redefine them. An overriding ambiguity permeates his essay. It is never clear whether his purpose is to argue that American imperialism was an accident or that America was not imperialist at all (it was never a unicorn at all, but a griffin). He nudges the debate in a new direction, to be sure, but he does not succeed in shoving the debate aside. It appears that, when the mail next brings "a copy of a new textbook on American diplomatic history," there, smack on the first page of its "worst chapter," will be that same old picture of a unicorn, a little more elaborately depicted, but the same creature we all know. Is the hope for a new debate a mere chimera?

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## Reply:

RARELY, I AM SURE, HAS SO ARGUMENTATIVE AN ESSAY been so politely received by critics who hold other views. And if what is not disputed can be assumed to be conceded (a perhaps somewhat optimistic assumption, given the commentators' limitations of space), I can hardly feel disappointed. But since there seems as yet no complete marriage of minds and since some of the arguments appear to slide past each other (as often in such exchanges), we should perhaps continue the discussion a little longer.

We may perhaps begin with specifics. I think that some of the authors on whom Walter LaFeber suggests I build might better be cited as representatives of the "on to China for economic hegemony" school of which he is so distinguished an exemplar. I quite agree that the dispatch of the *Maine* to Havana marked an escalation of pressure in the Cuban question; but since she presumably was neither sent to be sunk nor dispatched as a first step toward the Philippines, I do not see that her sailing made the Asiatic "New Empire" any the more probable. I thought the early examples of eastward-sailing Americans worth noting in view of prevalent assumptions that the term "'Far East' . . . hinders the understanding of American expansion [since] the United States has more often considered this area as the Far West" and that the *Empress of China* and Commodore Perry somehow support this conclusion.<sup>1</sup> The Athenian analogy seems to me more curious than enlightening: can it really be said that in 1897 American influence in Hawaii, the Philippines, and China (or, indeed, in Latin America) approximated Athenian control of Ionia in the decade before 450 B.C., that the Americans had already made off with the Delian treasury, or that the United States possessed "instruments of empire" like those described by Russell Meiggs?<sup>2</sup> On the other hand, the underlying argument that facts are more important than labels seems to me central: as Robert Beisner has elsewhere observed, "Behavior, not occasional rhetoric, is the crucial test."

At this point I think I should confess my sneaking admiration for the "new empire" school's concern with the interests and influence of nongovernmental groups, assuming it can be properly articulated and focused. But if we employ it without preconceptions, I doubt we will find pre-1898 pressures for trans-Pacific expansion, or even much pressure for the annexation of Hawaii. Theodore Roosevelt, at least, feared in the spring of 1898 that McKinley was incapable of dealing at once with Cuba and Hawaii, and that the president would let the islands slip away. Writing in 1900, Mahan noted of the expansionists that "their vision reached not past Hawaii, which also, as touching

<sup>1</sup> Walter LaFeber, *The New Empire: An Interpretation of American Expansion, 1860-1898* (Ithaca, N.Y., 1963), 5, 11, 34-35.

<sup>2</sup> Meiggs, *The Athenian Empire* (London, 1972), 205-19.

the United States, they regarded from the point of view of defense rather than as a stepping-stone." Thomas A. Bailey concluded that with no war there would have been no annexation, in which view he echoed Mahan: "To Dewey's victory, apparently, is due that we annexed Hawaii."

LaFeber refers twice to the "tools of empire" forged in the 1890s, to which I feel constrained to reply, what tools were these? Not the navy in the Pacific, certainly, and not Pacific merchant shipping (unless the words of Seward and Cleveland are still expected to do duty for real tonnage), and surely not a well-run consular service or a group of lively and energetic business men in hot pursuit of the China market.<sup>3</sup> He observes that historians have "proven beyond doubt" that it was because McKinley and his advisers (which advisers?), like their predecessors (Cleveland?), understood the problem of Asia so well that they could "move so rapidly into the Pacific." The argument is hardly supported by the Philippine reinforcement, an exercise in improvisation if ever there was one, in which the first troops did not sail until three and a half weeks and the monitors (not, perhaps, the best type of ship for this assignment) until six and a half and eight and a half weeks after Manila Bay. If Camara had gone the route, the race promised to be a very tight one, and the slowness of this movement "into the Pacific" was the one thing Mahan later thought culpable in the work of the Naval War Board. Problems such as these seem to me illustrative of the kind of cement that is employed to hold the "worst chapter" together: mutually supporting references ("beyond doubt") and a usage of assertion and verbal prestidigitation which imposes false geographic unities (interest in the eastern Pacific becomes interest in "the Pacific," a far larger area) and confuses words with things ("tools of empire" with ships, prompt orders with delayed ship movements).

This question of "new empire" behavior as against "new empire" rhetoric leads us back to James A. Scrymser. I said "books" and did not mean to single out LaFeber, and of course we all leave some things out. But the surprising fact is that *everybody* leaves Scrymser out, along with his energetic fellow-laborers in southern vineyards. Given all the talk of the American China Development Company, it is instructive to compare the sketches in the *Dictionary of American Biography* of Scrymser, Henry Meiggs (a great-uncle, by the way, of Russell Meiggs, who thus enjoys both inherited and acquired qualifications as an historian of informal empire), Charles J. Harrah, William R. Grace, and Minor W. Keith, with the notice given them in *The New Empire*, or in Milton Plesur's *America's Outward Thrust* (1971), or indeed in almost any book that comes to mind. This same problem of geographical displacement applies to missionary as well as business enterprise: we still get works that suggest that China was the only American mission field, yet as late as 1900 the four major missionary boards were spending half again as much money and supporting about half again as many workers in India, Burma, and Ceylon (hardly areas of American "imperial" activity) as they were in China.

<sup>3</sup> On this point, see Paul Varg, *The Making of a Myth* (East Lansing, Mich., 1968), and *The Closing of the Door* (East Lansing, Mich., 1973).

It also applies within major regions: Beisner affirms America's "pre-occupation" with Hawaii, China, the Philippines, and Korea (but at what dates?), yet makes no mention of Japan. But from 1895 to 1898 trade with Japan exceeded that with China, in 1898 in a proportion of three to two; the Japanese were buying such modern commodities as fertilizer and locomotives and electrical equipment; American missionaries in Japan were busy doing good and founding schools and colleges; and throughout the latter part of the century numerous individuals (in addition to the curious LeGendre) like Raphael Pumpelly, Horace Capron and William S. Clark, Ernest Fenollosa, David Murray, and Henry W. Grinnell were (I am sorry to have to say it again) in and out of Japan doing their things. This neglect of Japan is widespread in the literature. Why this geographical discrimination among outward thrusters? Or why the functional discrimination that concentrates on Denby and Wilson in China to the neglect of W. A. P. Martin, Philo McGiffin, and W. Pethick?

I am puzzled as to how to answer accusations of materialism and "almost total neglect of ideas," the more so, as I am simultaneously reproached for the use of "purely intellectual explanation." Certainly, I believe capabilities to be important. My critics are, of course, quite right in observing that the message on the cable is what starts things moving. Equally, however, if there is no cable to carry the message, Dewey will not get underway, and, if he has no coal, he cannot. I must confess to a slightly bruised *amour propre* in that Beisner did not find, in the continuous information flow made possible by cable communications, the explanation of the shift from "incidents" to "policy" in the "new paradigm" diplomacy which he propounded but never accounted for. In any case, I do not see how material matters and ideas can be divorced. Artifacts embody previous thought. Cables do not lay themselves, nor do freighters choose their own destinations, and the location of cables and shipping lines surely reflected contemporary views of what was important and what profitable.

On the question of accident in history, I can only feel that Beisner fails to understand me. Of course there are subtle and complex problems here, but in a one-paragraph treatment I would not think it necessary to explore them at length (any more than I would think it necessary at this date to heap up evidence to show America's social and cultural orientation toward Europe). The problem, it appears to me, is to get our minds out of the traditional boxes and to let them consider alternative possibilities (as those who were then running things surely had to do). So let me make one more attempt. As things were at the time, it was surely not too far-fetched to have hoped (as Cleveland and McKinley apparently did) that Spain might somehow manage to resolve the Cuban question on its own. If that had happened, would Beisner think we would have seen American troops fighting their way up to Peking in 1900? Or to take a fresh example: what if on that moonless night Dewey's navigator had piled the squadron up on the rocks while attempting to enter Manila Bay? Here, at last, we can profitably employ the Athenian analogy: if the com-

modore and his crews had suffered the fate of Nicias, what would the next decade have brought with regard to American policy in China?

To LaFeber's feeling that none should attack those who try to place the 1890s in a larger framework, and that it is "inexcusable" to say that much of their work is wrong or irrelevant, I would merely observe that I do not attack either the individuals or the effort, but merely the conclusions, and that criticism is one of the conventions of the profession. Beisner chides me for not having reformulated the entire argument and provided the entire answer. I think he asks too much of a single article. In any case he asks more than I undertook to do, which was to question some things that seemed to me wrong—although widely accepted—and to suggest some new ideas for consideration. The best chapter, like the "worst chapter" (if it is, in fact, the "worst chapter"), will surely turn out to be the product of collaborative effort.

IT MUST BE CLEAR BY NOW that much of my concern is with the monochromatic straight-line interpretation of the history of American foreign relations, which implies (in its most vulgarized textbook versions) that when Daniel Boone headed for Kentucky and Huck Finn lit out for the territory they were merely the advance guard of a relentless westward advance that, in due course, would cross the Pacific to end in the Cambodian incursion; and with the one-size-fits-all interpretation, which suggests that if only we repeat, like John Randolph's whippoorwill, "empire, empire, empire," everything from Jefferson to Nixon will be explained. The past is surely more interesting and diverse than this. So in conclusion I may perhaps be permitted to suggest "what is to be done," or at least a few things that I would like to see done, or not done.

It is true that I do not define "American imperialism" (nor do most writers on the period) nor, indeed, so far as I am aware does Beisner, although he uses the term liberally in both his critique and his book. In fact, I tried very hard not to use the word outside of quotation marks or paraphrase. I think it undesirable to begin with an assumption of an undefined "American imperialism": better describe the attitudes, the aims, the capabilities, and the events, and then see what they add up to. The question of "influence" is both important and difficult, but I doubt that snippets from the writings of historians and clergymen should be claimed as influential if they cannot be shown to be; here Karsten's observations on the influence of Mahan seem to me susceptible of wider application. As to Brooks Adams, I have to confess feelings of despair: I thought he was dead, but clearly he will not lie down, and it may turn out that he is immortal. Nevertheless, I must report that I find no evidence in the pages in Arthur Beringause's biography cited by LaFeber of Adams plotting strategy with Roosevelt and Lodge, nor have I been able to discover in Thomas McCormick's study of the China Market any reference to Roosevelt's carrying the good word from Adams to McKinley. In this context it may be worth noting that Roosevelt's prewar correspondence provides unflattering judgments on the quality of Adams's thought.

I think more attention should be given to negative evidence of the sort that Holmes understood but that the unimaginative Inspector Gregory did not ("Silver Blaze"): if the dog does not bark in the night, if presidents will not urge, if the senate will not consent, if congress will not dredge, if capitalists will not invest, if the navy will not deploy, the facts seem to me at least as important for an understanding of "American policy" as the pleas of enthusiasts. I think chronology important: what happens second can hardly cause what happens first (even though it may help to explicate it), and the current literature is far too rich in the use of post-Manila Bay quotations to define prewar aspirations. My critics speak of Mahan's views "from the mid-1890s until at least 1906," of the "decade of the 1890s," and of the "whole decade." Such chronological conflation seems particularly dangerous in periods like this. One may no doubt generalize about the years before 1898, and again regarding some period beginning in the summer of that year. But it appears to me that there is an important intervening break in which (to conflate Conant and Mahan) an accident at Havana and the flash of Dewey's guns first brought the Philippines above the mental horizon. I may not be able to persuade everybody, but I remain myself persuaded that unanticipated events can bring unanticipated results and that the *Maine* and Manila Bay (like Fort Sumter and Pearl Harbor) led to attitudes and consequences that could hardly have been foreseen.

One last thought on periodization as opposed to the straight-line view of history. Is it not possible that Seward's spread-eagle oratory, the "irreflective" re-establishment of the antebellum squadrons, the commercial concerns of Shufeldt and Evarts, and Chandler's call for lots of naval bases represent less a first step toward the Asian involvement of 1898 than the last manifestations of attitudes formed during the great age of sail and of the world-wide merchant marine and commerce-protecting navy? And that beginning in the late 1880s the new age of steam and of the navies of industrialism was finding its appropriate expression in concern for the defense of East Coast and Caribbean, in Mahan's emphasis on battleships and concentration of force, and in his growing realization that, although sea power may originally have derived from commerce, the missions of some modern navies (like the Russian and American) were primarily political?

My hope in this article has been to raise some questions and arguments, substantive and procedural, for consideration by the trade. If my colleagues (or at least those who are interested) will read it, and decide what they think persuasive and what not, then perhaps we can get back to the drawing boards and try to find out what the years preceding 1898 were all about, leaving the labeling until later. If this can be done the rubble question will solve itself: to the extent that they were founded upon rock, the pre-existing structures will presumably survive any winds that I or anyone else can blow against them; to the extent that they were built on sand, they may not, and perhaps should not.

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## Reviews of Books

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### GENERAL

PETER MUNZ. *The Shapes of Time: A New Look at the Philosophy of History*. Middletown, Conn.: Wesleyan University Press. 1977. Pp. xi, 382. \$20.00.

This intellectually stimulating book attempts nothing less than a reinterpretation of the nature and philosophy of history. In his lively introduction, Peter Munz asserts that ordinary (narrative) history is dying of inanition because overspecialization and academic emphasis upon fact-gathering have severed it from the philosophy of history. Munz acknowledges, and shows in later chapters, that the old historicist philosophies of history are invalid. But he argues persuasively that a viable nonhistoricist philosophy of history is both possible and necessary to restore historical research and writing to their former speculative function and significance.

Munz vigorously attacks the prevalent positivist notion that history is the study of an objective reality, which exists outside the minds of the creators of historical consciousness. His general thesis is that the philosophy of history, properly understood, is the culmination of a process by which consciousness makes use of universals to transform time into history. It is a process begun by the agents of history and continued by historians reflecting on the reflections of the agents of history, which is all that is available to historians. "Hence the difference between action, historical narrative of that action, and a philosophical understanding of that narrative is merely a difference in degree" (p. 19). In applying this thesis to the traditional problems of historical theory—causality, objectivity, interpretation, truth, and the relation of history to the social sciences and literature—Munz relies heavily, too heavily some will object, on R. G. Collingwood's controversial view that all history is the history of thought, the covering law theory advanced by Karl Popper and Carl Hempel, the structuralist methodology of Claude Lévi-Strauss, and the literary techniques used by H. V. White in his *Metahistory*.

Munz argues his thesis by showing, first, that it is not the sequence of events in time that enables the historian to link them meaningfully, but rather his use of general laws whose credibility is established by his historical milieu. Next he demonstrates that there is no absolute difference between explanation and interpretation, between primary and secondary sources, or between the historian and the historical actor. And, in one of the best chapters, he shows that the only important difference between history, fiction, and myth, all of which tell stories, is that history is more preoccupied with locating and relating events in space and time. Munz concludes from all this that there are no problems of history, but only problems in the writing of historical narratives; that some philosophy of history is implicit in all historical narratives; and that the social sciences and contemporary literary theory enable the historian to replace a historicist by a nonhistoricist philosophy of history.

Munz does not always do justice to opposing points of view, especially the *Annales* school of thought and Marxism, both of which he disqualifies as valid philosophies of history. Different as they are, what they have in common is their belief in a knowable objective reality, which flies in the face of Munz's own avowed subjectivism and relativism. Munz may be right to insist that we cannot know reality directly. But if we cannot know reality ultimately, if "the real reason why it [truth] must forever elude us is that it is not there" (p. 221), how are we supposed to choose, how do we in fact choose among equally plausible and consistent but different and competing interpretations of the past? And if our choices are arbitrary, what, then, is the point of history? Munz does not answer these questions, at least not satisfactorily. Nevertheless, few historians are likely to view their profession in quite the same way after reading this study. And that, after all, is a sure sign of an important book.

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R. N. BERKI. *The History of Political Thought: A Short Introduction*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. vi, 216. Cloth \$11.00, paper \$5.50.

The endeavor to provide us with "a short introduction" to the history of political thought seems modest enough. Still, it is an ambitious undertaking. For to do it well not only imposes severe constraints on loquacity (which can be to the reader's advantage) but also requires that a mass of material be compressed and selectively distilled, incurring thereby the danger of omission in scope and depth of its treatment of ideas, figures, and periods (to the reader's disadvantage). R. N. Berki is cognizant of these exacting conditions and successfully overcomes them. His style is economical and lucid and altogether readable; his expositions and analyses are concise and scrupulously organized so that it is remarkable how much he manages to survey and assess in this small book. He maintains "that the history of political thought can be rendered interesting and intelligible by being presented in a coherent summary form without too much simplification or reduction of its content." He proves his point admirably.

Two chapters precede the historical survey. The first is concerned to explain the derivation of political theory from political experience; the second discusses a number of philosophical and methodological issues resident in and induced by the notions of "history" and "tradition" in the study of political thought. Some of these issues are subjects of current controversies and contests over what is—and what is not—to be looked for in ascertaining the meaning, truth, and manner of interpretation of the text (and perhaps intentions) of the author of a political theory. These chapters form a stimulating and instructive preliminary introduction to the study of political thought and its history.

The main portion of the book is devoted to the history of political thought—the "visions" of the most influential thinkers. "Vision" is a quasi-technical term, a high-level reflection, descriptive and evaluative, on the nature of the state (pp. 10–13). The theorizing of political theorists—here their vision—is directed to the ends and purposes of political activity centered in the state. Berki's focus in the four chapters comprising this history is on certain basic historical conceptions of human nature and ideals in relation to the state and the fundamental changes in the nature and conception of the state. The visions then treated are: the philosophic vision of ancient political thought; the religious vision of medieval political thought; the civic and the social visions of early modern and modern political thought. The vision is a framework for

delineating the pervasive historical background as well as characteristic motives and assumptions affecting the procedures and conclusions of the major thinkers. They are all here: Socrates, Plato, Aristotle, Epicureans and the Stoics, Cicero, Augustine, Aquinas, Marsilius, Bodin, Hobbes, Locke, Rousseau, Burke, Hegel, Mill, and Marx. That Ockham or Kant, for example, are not included is regrettable but understandable in a short introduction. In addition to balanced and clear expositions of the theories, Berki manages to supply vital and illuminating bits of historical information and some brief critical diagnoses.

In sum, this is an excellent book within the limits the author has conceived for it. It will be of value certainly to students of political theory for whom it was primarily designed; but it is also recommended to any reader interested in an articulate and compact introduction to the history and substance of political thought.

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EDMUND IONS. *Against Behaviourism: A Critique of Behavioural Science*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. xiii, 165. \$11.00.

It is difficult, without sounding contentious and simple-minded, to argue that many social scientists are spouting nonsense—even if it is the case that they are doing so. Edmund Ions has run this risk in his little book. The nonsense that concerns him he calls "behaviourism," whose pernicious influence he charts in sixteen short chapters. Each chapter identifies some specialized group of behavioralists such as "Abstractionists," "Generalists," "Game Theorists," "Cross Culturalists," "Content Analysts," "Cliometricians," "Ethnomethodologists," and so on. Also included are pollsters, economists, B. F. Skinner, Chomsky, and Lévi-Strauss.

Ions never defines behaviorism, but what he seems to mean is the uncritical application of mathematical techniques in the study of social phenomena. Had he stuck to this the book might have had some force. But unfortunately, in chapter after chapter, Ions fails to distinguish between what he claims are the mathematical techniques of behaviorism and other forms of generalization. The result is a quarrelsome indictment of the application of any generalization at all. Such a radical thesis might be successfully maintained. But naively to assume that thesis without arguing for it, as Ions does, is hardly helpful or illuminating. He invokes Isaiah Berlin, Karl Popper, and Peter Medawar as his methodological patron saints but offers no sustained discussion of their views.

All this is a pity, for it leads Ions to beg the important questions. Here is one of his typical critical summaries: "We cannot judge whether the theory fits the facts, since, in terms of the vast number of facts available on such broad and complex themes, the fit, even the relevance of the selected facts, are extremely difficult to appraise. What is clear, however, is that the few facts supplied are made to fit the theory. In no visible sense does the theory fit the facts" (p. 82). But, alas, it is no longer a compelling criticism of a theory to complain that the facts are made to fit it; that theories determine facts is now too widely believed to be corrected by simply denying it. If this is a pernicious belief, as it probably is, it must be shown *how* it is pernicious, for that is far from obvious.

The same difficulties mark the chapter on behaviorism in historical studies entitled "Cliometrics." Ions takes up the work of Fogel and Engerman on slavery. But, surprisingly, he discusses not their major claims but their alleged misreading of the historiography of slavery. Even if Ions is right about the misreading, such a peripheral attack is disconcerting from someone whose main thesis is the abuse of mathematical techniques, especially since Fogel and Engerman have become notorious in many eyes for just such abuse. The remainder of the chapter is oddly devoted to Richard L. Merrit's study of German textbooks—a subject more appropriate to Ions' chapter on content analysts. In the end Ions says little about cliometrics or any other historical method. By indiscriminately attacking all forms of generalization, he has made his opponents seem like well-meaning victims of the human condition. But as his own anger suggests, the problem is deeper and more subtle.

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LIONEL KOCHAN. *The Jew and His History*. New York: Schocken Books. 1977. Pp. x, 164. \$15.00.

Lionel Kochan's *The Jew and His History* operates with categories which transcend the canons of Western, critical historiography. For him, fact is fancy, and fancy fact. The sages of the Talmud, in Kochan's view, were far better historians than the Graetzes and Dubnows precisely because the former regarded facts as irrelevant, while the latter regarded them as the very stuff of history. This bold rejection of facts, accompanied as it is by the no less bold assertion that the Talmud's use of history should be paradigmatic for Jewish historians, is forcefully underscored by Kochan in the concluding pages of his work: "The strength of the

Talmud's use of history lies precisely in the fact that it makes possible the advance from critique to *praxis*. It thus averts the degeneration of historiography into what Nietzsche called 'a picture gallery' . . . . As against this contribution of the Talmud, what can history offer, especially the messianological history of the mode practiced by the historians discussed earlier? *Only fantasy*" (p. 117, italics mine).

The starkness of these affirmations can be fully appreciated only when one is aware that the sages of the Talmud had no regard whatsoever for historical facts. For them, whatever had been revealed in sacred writ, and whatever had been affirmed by authoritative teachers was deemed to be historical truth. As a consequence, the Talmud is a repository of imagined events, nonoccurrences, and freely constructed paradigms of model and not-so-model lives. The Talmudic teachers filled the past with events that did not occur, laws that had not yet been given, and ideas that were not to be born for centuries. Indeed, these teachers were so free of factual constraints that they did not hesitate to redraw the portraits of Abraham, Moses, David, and all other biblical heroes so that they could hardly be distinguished from the Talmudic sages themselves. Concerned as they were with ultimate transhistorical reality, these sages looked to this source for their "facts," and for a history above event, occurrence, or happening.

Since Kochan subscribes to this "higher" concept of history, he lifts his thesis above criticism, even as he disposes of those Jewish historians who were under the illusion that fact was not fantasy and fantasy not fact.

Reading this book is thus a bewildering experience, since the rise of critical historiography is pictured as a fall from noncritical historiography. As such, *The Jew and His History*, like Augustine's *City of God*, would have us believe that the real history of the Jews was the nonevents, the nonoccurrences, and the nonhappenings which the Talmudic sages so freely imagined as though they had been events, occurrences, and happenings.

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SAMUEL Y. EDGERTON, JR. *The Renaissance Rediscovery of Linear Perspective*. New York: Basic Books. 1975. Pp. 206. \$15.00

In this short book the technical history of perspective is but one of several themes. First there is the chronology, starting from Greek optics and perspective in relation to Greek theatre and Roman architecture, then on to Arabic texts on optics and medieval Europeans (Bacon, Pecham). Then

there is a discussion of the "discovery" of linear perspective, as in paintings by Brunelleschi and of its exposition by Alberti. So much is orthodox history. Samuel Edgerton goes beyond the traditional accounts by reconstructing and performing the experiments that led Brunelleschi to his discovery. These reconstructions are very convincing and constitute an important addition to our knowledge of this important discovery.

Edgerton insists that it was Brunelleschi's discovery of the "vanishing point," the chief fruit of his experiments, that caused the new system to become so powerful, especially in the hands of Alberti, and to transform art. Hence Edgerton devotes three of his eleven chapters to Alberti. Brunelleschi left no corpus of writings on perspective, and his actual stages of discovery have had to be reconstructed. Basically, Edgerton's thesis is that Brunelleschi found the vanishing point experimentally and that Alberti (whose writings do survive) then developed the subject mathematically.

Edgerton adds to this history the figure of Ptolemy, the astronomer, physicist, and geographer of the second century A.D. Ptolemy had developed ideas of perspective in his mathematics of map-making, his methods of "projecting" the spherical surface of the earth onto a plane surface (or, the equivalent, his way of looking at a sphere so as to transform it into a plane). Here Edgerton found a valuable clue in a 1959 article by O. Neugebauer, who reconstructed Ptolemy's method of double orthogonal projection. Edgerton's interpretation of Ptolemy's map projections in relation to the history of linear perspective (what he calls "a cartographic method of linear perspective") is the reason for his reference to a "Renaissance rediscovery of linear perspective." Edgerton would, additionally, have Ptolemy be responsible for the method of "grids" or ruled squares used in architectural plans in the Renaissance, and he concludes that "Ptolemy's *Geographia* may be a kind of missing link between Brunelleschi's architecture and his linear perspective investigations." On this score the critical reader must give the Scottish verdict of "not proven," but I believe there can be no doubt that cartographic methods were associated in some way and to some degree with the rise of perspective in the Renaissance.

Edgerton is not content merely to make a somewhat technical contribution to the history of perspective. He is also concerned with perspective in relation to the social and religious functions of art. In particular, he explores the extent to which seeing in perspective may be only a convention, one of many possible ones. Here he associates himself with the intellectual or philosophical position adopted by Erwin Panofsky, although he ac-

knowledges Panofsky's error in assuming (among other things) that because "the human retina is . . . a concave surface, we . . . tend to see straight lines as curved." Panofsky expressed his general view of perspective in the title of his essay, "Perspective as 'Symbolic Form'." It will surely seem curious to anyone interested in the intellectual history of the twentieth century that Edgerton goes to such lengths to explain that this term "is not original with Panofsky at all," but was adopted by him from the writings of Ernst Cassirer.

Among Edgerton's challenging theses, one of the most arresting is that perspective did not arise in the Renaissance because of a Renaissance "fascination with truth," as has been alleged, as if perspective was a device for making a "true" equation of what is seen and what is depicted. Edgerton rather would have perspective be "a means of—literally—squaring what was seen empirically with the traditional medieval belief that God spreads his grace through the universe according to the laws of geometric optics."

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DONOVAN H. BOND and W. REYNOLDS MCLEOD, editors. *Newsletters to Newspapers: Eighteenth-Century Journalism*. Morgantown, W.Va.: School of Journalism, West Virginia University. 1977. Pp. x, 318. Paper \$10.00.

Journalism during the eighteenth century advanced to an unprecedented level of maturity and sophistication in terms of its role and function in society. While its antecedents can be traced to the introduction of movable type by Gutenberg in the fifteenth century, and beyond that to the *Acta Diurna* of Rome before the birth of Christ, its modern definition derives from its development after 1702. The century saw the birth and success in England and America of daily newspapers, magazines, and specialized periodicals. It saw the birth of a profession that responded to a steadily growing audience during a time of far-reaching political, social, cultural, and economic change. Historians of journalism should find this volume of papers (originally presented at a Bicentennial Symposium held at West Virginia University in the spring of 1976) a valuable asset to their research. Additionally, because one cannot separate the texture of journalism history from the fabric of the times, the volume should command a wide readership among historians in general.

The topics and ideas explored in the papers are as rich and diverse as the period with which they deal. Among the subjects covered are the role of

the journalist as "*censor morum*" in English and American society; the concurrent development of journalism and literature, especially in the writings of Richard Steele, Jonathan Swift, Daniel Defoe, Samuel Johnson, and Benjamin Franklin; the activity of women as correspondents, editors, and printers, and the identification of them as a specialized audience, particularly for magazines; and the effect of post office policy on the establishment of a communications network on the North American continent. In addition to focusing attention on topics which until recently have not commanded the degree of study they deserve, several of the papers reexamine and revitalize more traditional topics, e.g., political partisanship and freedom of expression, either by taking a fresh look at the documentation already available or by bringing to light new evidence.

Henry L. Snyder's study of English newsletter writers is painstakingly researched and highly informative. Jean E. Hunter's examination of the changing status of English women as reflected in the pages of *The Lady's Magazine* should spur further study in this area. Although colonial printers eventually united in opposition to the Stamp Act, Francis G. Walett argues convincingly that many were prepared at first to pay the tax. This contention receives support from William F. Steirer's findings that Philadelphia printers moved slowly, albeit intentionally, toward politicization after 1764. Kent R. Middleton presents a strong case for the thesis that the protection of "commercial speech" was very much on the minds of the framers of the First Amendment because of its relationship with property rights.

The volume is divided into four parts with each part encompassing roughly a quarter of a century. Because of the diversity of the topics the editors probably made the correct structural choice. Unfortunately, the editors' introductions to each part are merely perfunctory, and although this does not detract from the value of the individual papers, the volume as a whole would have certainly been enhanced by a greater effort to set the context and the tone for each section. Nevertheless, the quality of the individual papers more than compensates for this weakness and makes the volume worthy of a scholar's attention.

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B. N. MIRONOV and Z. V. STEPANOV. *Istoriĭ i matematika (Matematicheskie metody v istoricheskom issledovanii)* [The Historian and Mathematics (Mathematical Methods in Historical Research)]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1975. Pp. 183. 59 k.

"We live in an age of mathematics" (p. 1). With these words, B. N. Mironov and Z. V. Stepanov open their brief handbook on quantitative methods in history. The authors argue that increased use of quantification is necessary if the quality of historical work is to be improved. They see the quantitative approach as central to the utilization of social science methodology, especially the latter's emphasis on formulating problems and posing and confirming hypotheses from empirical data. Further, they claim that quantification uses the same principles of logic as traditional history, in this way hoping to defuse the resistance of traditional historians to the quantitative method.

The handbook explains the mathematical logic of selected statistical measures, outlines the conditions under which those techniques may be used, and gives detailed examples from the work of Soviet quantitative historians. The authors range from descriptive statistics to the more complex multiple regression. They used work on the causes of inflation under Catherine II and VSNKha decision-making in the 1920s to explain quantitative indicators or qualitative data. Sampling is made clear through references to the work of I. D. Koval'chenko on the serf economy in the first half of the nineteenth century and Iu. Iu. Kakhk's study of the Estonian peasantry in the same period. One-third of the book deals with correlation analysis, seen as the most useful quantitative tool. Such measures are presented through studies of the development of a national market and the collective biography of participants in the revolutionary movements under Alexander II. By using a dozen factors to look at grain prices in late nineteenth-century Russia, the authors briefly present multiple regression. Measures involving rank-ordering, especially Spearman's *rho* and Kendall's *tau* also are employed to analyze the revolutionary movement, while measures of variance are explained by looking at *pomeschik* income in the nineteenth century.

Though clearly explaining statistical methods useful to social and economic historians, Mironov and Stepanov do not go beyond established research, they offer no innovative methodology. Still, in the absence of any handbook suitable to their needs, American students of Russian history may find *The Historian and Mathematics* useful in their own work.

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LARRY LAUDAN. *Progress and Its Problems: Towards a Theory of Scientific Growth*. Berkeley and Los Angeles: University of California Press. 1977. Pp. x, 257. \$10.95.

Larry Laudan's *Progress and Its Problems* is the latest major addition to the historiographic debate that began fifteen years ago with the publication of Thomas Kuhn's *The Structure of Scientific Revolutions*. As such it is highly recommended to anyone curious about the direction of the debate since the appearance of Kuhn's classic monograph. Intellectual historians should be especially interested, since Laudan explicitly draws his conclusions beyond the history of science to the history of ideas generally.

Being a work in historiography rather than history proper, Laudan's book is more philosophical than historical. But it is philosophy unencumbered by esoteric philosophical techniques or vocabulary, let alone special symbolism. Conclusions are stated and arguments given in clear, forceful prose, and most points are illustrated with a variety of historical examples. One may disagree with the conclusions or reject the arguments, but one is rarely left wondering what Laudan claims or why.

According to Laudan, the historian of ideas (scientific or not) should seek, not merely to explicate the thoughts of previous thinkers, but to explain why they believed and said what they did. Moreover, the one definitive form of explanation open to the intellectual historian is that the people in question believed what they did because that was the *rational* thing to do. Only if it is determined that the beliefs were not rational is it proper to seek explanations in the individual psychology or the social situation of the thinkers involved. Clearly such a view presupposes a theory of rationality. Thus most of this book is devoted to developing a theory of scientific rationality adequate to the needs of the historian of scientific ideas.

On Laudan's account, the historian of ideas should focus not on ideas as such, but on problems. When the subject is science, there are two basic types of problems to consider: empirical and conceptual. Roughly, "anything about the world which strikes us as odd, or otherwise in need of explanation, constitutes an empirical problem." It follows that there cannot be empirical problems except against a background of theory that makes some things "odd" or "in need of explanation." Laudan thus agrees with Kuhn and others in rejecting the empiricist philosophers' neat dichotomy between "theory" and "experiment." But he goes further in that he would also have us judge theories by their ability to solve *conceptual* problems. These include both conflicts among the concepts internal to the theory and conflicts between the theory and other systems of concepts.

Laudan also distinguishes between theories and research traditions, e.g., between Newton's mechanics and Newtonianism. This distinction was blurred in Kuhn's notion of a "paradigm" but has

been developed by a number of more recent authors, especially Imre Lakatos. Laudan's overall view, then, is this: the rational acceptability of a *theory* is a function of its ability to maximize the number of important problems solved and to minimize the number of anomalies and conceptual problems created. The acceptability of a *research tradition* is a function of the acceptability of the latest theories in that tradition. Since, for Laudan, progress is a function of the number of important problems solved, scientific rationality is thus defined in terms of scientific progress. Science is not progressive because it is rational; it is rational because it is progressive.

Historians have understandably tended to be wary of global theories of rationality assumed to be applicable in all places at all times. As Laudan quickly notes, however, it is only the general form of his theory that is global in this sense. Any application requires that one look to the specific historical context in order to fill out the relevant parameters, e.g., the set of problems regarded at the time as most in need of resolution. One need not fear that Laudan's account of scientific rationality is overly restrictive.

Finally, Laudan explicitly argues that "determinations of truth and falsity are *irrelevant* to the acceptability or the pursuability of theories and research traditions." While such a view follows recent trends, others have seldom been so explicit in banning the concept of truth, even as an ideal, from their historiography. By drawing the lines so sharply, Laudan has insured that his will not be the last word in this debate. But that was not his purpose. His aim was to offer "a fresh perspective" on these problems, and that he has done.

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HARRY F. DOWLING. *Fighting Infection: Conquests of the Twentieth Century*. Cambridge, Mass.: Harvard University Press. 1977. Pp. vii, 339. \$15.00.

Harry F. Dowling, now a visiting scholar at the National Library of Medicine, comes to this task with impressive credentials. As Clinical Professor of Medicine at the George Washington University School of Medicine and head of the departments of Medicine and Preventive Medicine at the University of Illinois, he has been an active participant in and contributor to the fight against infection. Moreover, Dowling has had a long interest in the history of medicine, and he writes with a feel for history and a mastery of its methodology.

This well-organized, well-written, fully documented, indexed study is readily comprehensible to the understanding layman: it has not been writ-



ten particularly for the physician or scientist. Dowling describes the identification, prevention, and treatment of virtually all infectious diseases, with tuberculosis and venereal diseases rating separate chapters. He discusses the role of the public health movement in the control of infectious diseases and recounts the efforts at immunization and treatment with serum in the first third of this century. Most of the book deals with the development of antibiotics—the sulfonamides, penicillin, and streptomycin are each treated in a separate chapter—and the newer vaccines.

Dowling is constantly aware of the broader social implications of his topic. To him, for example, tuberculosis involved not only the identification of the micro-organism and the search for vaccines and chemotherapeutic agents, but was, for two decades, an instrument of death for a hundred thousand people and illness for another million people per year in the United States. Similarly, he recognizes the impact of “societal changes” on the transmission (and raising the virulence) of the micro-organisms involved in venereal diseases. Indeed, his discussion of venereal disease is social history: prostitution, sex instruction, public and private campaigns, public policy, sexual freedom, oral contraceptives, and homosexuality are all brought into the story.

The role of social organization in the process of learning about and conquering infectious diseases is evident, although implicit rather than explicit, throughout the book. Military discipline and control of large numbers of men played an especially significant role; hospitals, schools, and African miners all offered opportunities for clinical trials. Without such opportunities and controls—both societal and clinical controls—the work of the scientists would have been impeded, if not impossible, and the health of society seriously affected.

Dowling’s approach is neither opinionated nor polemical. One has to go to his *Medicines for Man* (1970) for his critique of the regulatory, industrial, and professional aspects of drugs in our society. When he does venture a judgment in the present volume he does so in a pleasant, understated manner. Moyer (the Department of Agriculture scientist who patented certain processes in the manufacture of penicillin in Great Britain) was “said to become rich,” Dowling says, but “Florey’s name will be written in the angel’s book . . . as one who loved his fellow men.” And again, Fleming, of whom other writers have been quite critical, “did the Oxford group a favor by monopolizing the limelight.” And Watson, Crick, and Wilkins “must have silently thanked Griffith and Avery for ignoring immediate practical goals to study the basic biology of the pneumococcus” when they received the Nobel Prize.

There are imposing statistics presented on the effect of the “conquest” on morbidity and mortality. (It is sometimes difficult to accept success when deaths were reduced to “only twenty percent,” as in the case of the use of streptomycin in meningitis.) Yet Dowling recognizes the failures, the limitations, and the future problems in the discovery and use of vaccines, serums, and drugs. He is careful to point out that morbidity and mortality rates were already in decline for several diseases even before the introduction of the modern prophylactic and therapeutic approaches. Clinically, this reflected the changes taking place in the microorganisms and in the host-parasite relationships—changes that continue to make “conquest” a never-ending process. Socially, it reflected improvements in public health measures, standards of living, and personal hygiene.

Finally, Dowling has pointed up some of the social implications of the conquests of infectious diseases: the changing age distribution of the American population; the size of the American family; the American sense of security and progress (the book has a largely but not exclusively American orientation).

Dowling is a medical historian with a keen sensitivity for the social implications and interrelations of his topic. The social historian, whose interests would lie more with the social patterns and changes that are influenced by these interrelations, will find this book an authoritative, comprehensive, and understandable study that he or she dare not overlook.

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HARRIET ZUCKERMAN. *Scientific Elite: Nobel Laureates in the United States*. New York: Free Press. 1977. Pp. xv, 335. \$14.95.

In 1896 Alfred Nobel left a legacy of approximately nine million dollars to be divided into prizes to honor those scientists who made important discoveries in physics, medicine, chemistry, and physiology, as well as to honor writers who created outstanding literary works, and finally to reward those who worked for fraternity between nations. There can be little doubt that the prize was originally intended to advance the growth of scientific knowledge and to foster idealism and humanity. While it has not substantively advanced these goals, it has, in the past three quarters of a century, become a symbol of extraordinary achievement. To receive a Nobel Prize is comparable to achieving a foothold on Mount Parnassus and the immortality that is denied to most men.



Harriet Zuckerman is one of that happy band of sociologists who, in recent years, have been studying science as a social institution. In her book, *Scientific Elite: Nobel Laureates in the United States*, she maintains that the Nobel Prize has become an important element in the stratification of science and, in fact, has tended to create a super elite. Unlike some political and economic elites, this particular elite is nonhereditary, and while it has enormous prestige and can exercise power, the power it does wield is exercised individually rather than as a group. Zuckerman's central concern is the development of this elite. Her analysis includes an examination of the social and educational origins of Nobelists, the process of upward mobility of those promising young scientists who in later life become Nobelists, a study of the holders of the so-called 41st chair (those scientists who have contributed as much to the development of science as Nobel Laureates, but who for a variety of reasons never received the Prize), and finally, the impact of the Nobel Prize on the subsequent scientific production of prize winners.

In the course of her study Zuckerman illuminates a wide variety of problems, from the social ingredients that tend to create an over-representation of Jews among Nobel Laureates, to the factors that lead future prize winners to study and work with Nobel Laureates or the holders of the 41st chair. In addition, she destroys a number of myths about creativity in science. For example, she demonstrates, beyond cavil, that Laureates typically do the research that brings them the prize, not while they are young, but rather when they are in their late thirties or early forties. Her key achievement, however, is providing a model of the social processes that tend to produce an accumulation of advantage in science. Still for all of its virtues there are a number of lacunae in this important pioneer study. Zuckerman has neither examined the state of the art of various scientific disciplines at the time Laureates did their prize research nor analyzed in any detail the peer-review bureaucracy of foundations and government. These bureaucracies, often manned by scientists of lesser achievement, are central to an understanding of the recognition and support of contemporary scientific research.

Much of Zuckerman's research for this work involved personal interviews with Nobel Laureates. Her account of the procedures used in interviewing this elite adds to the techniques already developed by Thomas Kuhn and Charles Weiner in their respective interviews with eminent physicists. There is, however, one caveat this reviewer wishes to express. It is high time that oral historians, as well as sociologists, spoke of the importance of the preparation their subjects must do for

such interviews. Memory must be refurbished. Unfortunately, printed materials go but a little way in preparing scientists for interviews. Without the detail supplied by correspondence, grant proposals, reports, protocols of experiments, diaries, photographs, as well as the testimony of contemporary laboratory assistants, one can hardly understand the process of scientific research, much less aid the memory of the scientist being interviewed. To urge such preparation is not a counsel of perfection: it is the only way one can truly get at the roots of contemporary memory.

There can be little doubt that historians and sociologists who in the future examine the stratification of science will return time and again to Zuckerman's study. I hope that in subsequent editions two niggling errors will be corrected. On page 92 Zuckerman states that Julius Axelrod discovered prostaglandins. That work was done by Ulf von Euler who received the Nobel Prize the same year as Axelrod. In other places, Zuckerman loosely states that Enders, Robbins, and Weller received their Nobel Prize for discovering "how to culture polio virus in test tubes" (p. 98) or again for "their method of growing polio virus in vitro" (p. 166). Albert Sabin and Peter Olitsky cultured polio virus in vitro as early as 1935. Sabin and Olitsky's research, which was meticulously carried out, suggested that polio virus could only be grown in cultures composed of nervous tissue. In 1949 Enders and his colleagues showed that polio virus could in fact be grown in non-nervous tissue as well. It was this discovery that opened the door to the subsequent development of polio vaccines.

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OTTO NEULOH, editor. *Soziale Innovation und sozialer Konflikt*. (Studien zum Wandel von Gesellschaft und Bildung im Neunzehnten Jahrhundert, number 15.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 273. DM 56.

The ten essays in this volume divide into two distinct categories: those that explore the abstract relationship between social innovation and social conflict, and those that investigate concrete historical situations in terms of sociological theory. Basically the social scientists contributing to this volume are seeking to broaden Joseph Schumpeter's emphasis on entrepreneurial innovation to include the state bureaucracy and pioneering ideas in social welfare. Thus Otto Neuloh, the editor, singles out for praise Friedrich Syrup, a career administrator who nationalized unemployment insurance during the Weimar years. In general any personage who sought to defuse social conflicts by new

approaches fits under the benign rubric of social innovator. This seems harmless enough until one finds the list of praiseworthy personalities includes such conservative luminaries as Freiherr von Stumm-Halberg and Graf von Posadowsky-Wehner in addition to Frederick Taylor (of the Taylor system) and the Zentralverband deutscher Industrieller. That all of these innovators were striving to maintain and strengthen essentially authoritarian systems of power is played down in the essays by Neuloh, Junius Wolfgang, and Gundolf Scheweling. The latter's effort to rehabilitate Taylor as the visionary behind cooperative post-World War II labor-management relations is particularly unconvincing. The footnotes would indicate that Scheweling does not read English and thus the major works on Taylor by Haber, Nadworny, and Kakar are ignored as are the untranslated works of Taylor himself. Another questionable endeavor of several essays is the effort to find the pedigree of *Mitbestimmung* in the social legislation of Imperial Germany.

Historians will be most interested in two contributions on nineteenth-century strikes and H. Volkmann's study of social protest in Biedermeier Germany. Ulrich Engelhardt finds that the failure of a miner's strike in Waldenburg (1869) did not lead the participants to abandon social liberalism for a more conflict-oriented theory. Only the prolonged depression after 1873 broke the hold of a set of beliefs emphasizing harmony. Albin Gladen persuasively argues that miners in the strikes of 1889 were still in the grip of *konservative Denkkategorien*, trying to restore rights that had been abridged with industrial development. Their sense of status decline is shown to be acute. Perhaps the most fascinating contribution is Volkmann's attempt to compare the level of social protest in German states during the turbulent years from 1830-32. His results will surprise historians who have stressed the social dislocation caused by industrialization. Overwhelmingly the states experiencing significant violence are the most reactionary in terms of political participation, tariff policy, agrarian reform, and guild restrictions. That is, the states with *Gewerbefreiheit* and low tariffs were relatively calm, leading Volkmann to see economic modernization as enhancing rather than weakening political stability. Of course, the short timespan (two years) of the study and the distortions caused by Prussia's size (Prussia having been quiescent and advanced in all areas except the constitutional) makes one hesitant to draw broad conclusions until further research is completed. Nevertheless, Volkmann has applied Charles Tilly's methods to Germany with stimulating results.

The mixed quality of the essays illustrates the difficulty of exploring the past in terms of sociolog-

ical theories; but since this worthwhile undertaking has only just begun in Germany the fruits will, no doubt, improve with further efforts.

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ALAN WOLFE. *The Limits of Legitimacy: Political Contradictions of Contemporary Capitalism*. New York: The Free Press. 1977. Pp. xv, 432. \$14.95.

This is a huge, rambling, inchoate, badly written, and totally disorganized book. One supposes that Alan Wolfe's aim was to present a history of the modern "capitalist" state, but there is no *history* here. Names are scattered almost at random throughout the work, but no one acts or pursues goals; everyone is simply there to exemplify and to play his appointed role for the *real* actors behind the scene: the powerful reified villains who prowl and interact, who grow and decay. In short, this is a work in the current Marxian mode; highly pertinent to Wolfe is Rondo Cameron's critique of Immanuel Wallerstein's *The Modern World-System*: "The outstanding methodological feature of the book, apart from its neomarxism, is its use of reification and teleology. For Wallerstein national states and social classes are not abstractions but real entities with lives and wills of their own" (*Journal of Interdisciplinary History*, 7 [1976]: 142).

Wolfe's dubious contribution to this literature is a plethora of wholly fictive "states" which grow, "disaccumulate," and interact throughout the book: the Harmonious State, the Accumulative State, the Franchise State, the Dual State, *et al.* But the overarching reified villain in Wolfe's demonology is "late capitalism": virtually every person or event is merely an embodiment of this entity. Thus, Richard Nixon, predictably, exemplifies "the schizophrenia characteristic of the political process of late capitalism" (p. 321). Under the circumstances, one is thankful that we never find out why Wolfe calls Robert McNamara "the Martin Luther of late capitalism" (p. 320).

The use of the term "late" of course heavily implies that capitalism is inevitably destined to pass rather quickly from the scene. But here Wolfe begins to hedge his bets, for apparently historical lateness can become almost interminable: "Nothing will be or can be suggested here that indicates how long the period of late capitalism is likely to last. . . liberal democracy may be in its last stage, but that stage has an indeterminable length" (p. 253).

True to the Marxian tradition, Wolfe's garrulousness disappears when it comes to describing the socialism of the future. We learn that all of life

will be politicized in an expansion of democracy. Curiously, in all of Wolfe's lengthy discourse on states, there is not a single reference to any of the real socialist states of the twentieth century. There is only the barest hint that not all contemporary socialist states may be fully democratic—not much of an assurance to anyone skeptical of what our expanded democracy is going to look like.

But to paraphrase Wolfe's own jargon, his book is only interesting as a manifestation of late Marxism, of New Left Marxism in decay. Gone is the early Marxists' hard analysis of actual historical events; gone, too, is their confident optimism. Where, in Wolfe's analysis, is the agency of social change? Who is going to slay the behemoth of late capitalism? Gone is High Marxism's working class and its vanguard party of professional revolutionaries; instead, the next period of history is supposed to be ushered in by leftists engaging in "electoral activity at the local level" (p. 345). Verily, the mountain has labored and brought forth a mouse. If this is all "it" has to worry about, late capitalism is in fine fettle indeed.

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HUGH TINKER. *Race, Conflict and the International Order: From Empire to United Nations*. (The Making of the Twentieth Century.) New York: St. Martin's Press. 1977. Pp. 157. \$16.95.

"Whoever discovered America deserved to be dragged home, in chains, to die." Thus wrote James Baldwin, perhaps echoing the feelings of many subjected or once-subjected people. It is an apt sentiment to bring to a study of the international dimensions of race.

The series of which this book is a part, "The Making of the Twentieth Century," is designed to provide an introduction to various topics; it also makes pretenses to be of value for advanced study, but these are not justified here. The book treats the consequences of the effects of white power and wealth as they were used to subjugate nonwhite people. During the last century this domination went unchallenged; but as the twentieth century unfolded white supremacy was confronted, according to Hugh Tinker, by the nations of the Third World, revolutionary Communism, and the depressed nonwhite minorities of wealthy white countries. His thesis, if not original, is nonetheless important. Race relations presuppose conflict; their resolution has been "revolutionary" in the sense that the nineteenth-century world order and belief system have been overthrown. Thus, in terms of race, a Gobineau or a Kipling would not recognize the world of the 1970s.

Race is a vital subject both historically and for our contemporary world; witness, for example, racially oriented international turmoil in Panama, South Africa, Rhodesia, and Palestine. But, although the book begins with promise, it quickly metamorphoses into a simple narrative summarizing the "high noon" of Europe's colonial empires, their demise, and the emergence of new nations. The analysis of race and conflict is too often sacrificed to a chronicling of events.

There is a plethora of factual information presented in terse reportage style. One finds nothing that will attract the scholar—no incisive analysis, no new material—though the undergraduate seeking a concise survey will find the book easy to digest. Most of the narrative reads like the news summaries that appear in almanacs or turn-of-the-year editions of major newspapers; of necessity, it is sketchy and spotty. Given the editorial bounds within which Tinker had to work (i.e., 135 pages of text, two centuries, a global geography), he had no alternative to brevity. Aspects of the problem of race, conflict, and the international order are enunciated clearly enough, but there is precious little evaluation.

It may be unfair to judge a book by what it lacks. Nevertheless, the absence of an analysis of such phenomena as the role of revolutionary nationalism or the impact of Marxism on Third World countries in relation to race seems unforgivable. And the missed opportunity of evaluating the influence of some key individuals is too glaring to ignore; for example, only indirect reference is made to Fanon and Senghor, while Cabral and George Jackson are nowhere to be found—these men, among others, in their lives and words had much to say about race and international conflict.

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EDWARD CORNISH. *The Study of the Future: An Introduction to the Art and Science of Understanding and Shaping Tomorrow's World*. Washington: World Future Society. 1977. Pp. viii, 307. \$9.50.

Time present and time past  
Are both present in time future  
And time future contained in time past.

These words by poet T. S. Eliot apply not only to our contemporary age but to any time and underscore one of the fundamental characteristics of the field of history, for history is the study of the dynamic interrelationship of past, present, and future, regardless of the temporal perspective from which one views it. Historians should begin to recognize the importance to them of the relatively new field of futuristics, and this work, the best

general book on that subject, is an excellent introduction.

*The Study of the Future* is the product of the collective work of a coterie of thinkers and researchers coordinated by Edward Cornish, president of the World Future Society. It contains a fine discussion of the "discovery of the future" and traces the evolution of attitudes toward the future through the history of Western man. It follows by examining the Toffleresque visions of trauma brought on by overly rapid change and the variety of alternative interpretations given by futurists to these changes. Finally, it concludes with several chapters about the field of futuristics itself, looking in the process at representative persons and institutions engaged in futuristics and stressing the problems and methodologies involved. The only difficulty in organization is that the book's extensive use of case studies occasionally obscures the analytical thrust of the argument.

Following the text is a series of appendixes citing important centers, associations, and periodicals in futuristics; an index long on personal references though weaker in subject references; and a bibliography excerpted from the larger list in the World Future Society's excellent sister compendium *The Future: A Guide to Information Sources*. The primary fault of the bibliography is its listing of only a few science fiction novels, selected apparently at random from the large pool of excellent science fiction sources.

The authors assume that the future will be a time of increasingly rapid change, a premise challenged in recent years by the adherents of the "Limits of Growth" and appropriate technology movements. Further, they postulate a world inseparable into discrete segments. Interdependence is the watchword; the world of the past, divided into national units or temporal periods, will not, argue Cornish and his colleagues, continue indefinitely into the future. The book does assert that humans, in collective groups, *can* shape the future for the better despite the problems caused by rapid change. The message is optimistic; the basic emphasis on human rationality and educability remains intact.

Defining futuristics as "history seeking to look forward instead of back," the book implies the integral connection between history and futuristics. Though it leaves explicit comparison to the reader, it does note that both fields are concerned primarily with the process of development, the "lively sense of human evolution through time." Further, both fields are based upon creative thinking and both demand intelligent extrapolation from data. Finally, both are fundamentally interdisciplinary and require a broad understanding of a range of more specialized disciplines.

Valuable as the book is, it is limited to a Western perspective and does not attempt to explore attitudes toward the future in non-Western contexts. In addition, it ends on a highly optimistic but historically naive note by suggesting that increased communications among concerned world citizens will overcome the constraints of human intransigence, short-sightedness, and institutional red tape. Communications are clearly a step in the right direction, and the course suggested by the book would surely aid in meeting the future intelligently. But, my own optimism notwithstanding, I think a greater attention to the barriers to global cooperation might have made the book's closing arguments appear less utopian. These criticisms aside, *The Study of the Future* is by far the best book on its subject yet published. Solid in its sources, well written and timely, it speaks directly to our own widening sense of the place of the historical profession in the understanding of human affairs. It deserves to be on every historian's bookshelf.

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## ANCIENT

CHESTER G. STARR. *The Economic and Social Growth of Early Greece, 800-500 B.C.* New York: Oxford University Press. 1977. Pp. 267. \$12.50.

The opening chapter of this book assesses evidence, and warns against imposing modern theories on a society which lacked the concepts of labor and capital, but had concepts unknown to us, such as *polis* and *chremata*; it observes that economic tensions in early Greece should not be explained simplistically as class struggle. The second chapter inquires into causes of growth in the period studied. Chester Starr dismisses facile explanations, such as trade, the introduction of coinage and the rise of tyrants, and examines more comprehensive factors: the development of the whole eastern Mediterranean area in the early Iron Age, political changes in Greece (the emergence of the *polis*), religion (the author notes the far greater number of temples and statues erected in the sixth century than in the seventh and uses this persuasively as an approximate measure of growth), demographic factors (Starr treats with skepticism, though not with complete denial, the hypothesis of an increase in population), and the outlook of the elite.

Subsequent chapters deal with overseas trade (S. argues that trade developed in bulk-commodities, not merely in luxury items), industry, and cities and coinage. The author makes the significant point that alongside aristocrats there were



"semi-aristocrats," independent farmers like Hesiod, men who could be required to serve as hoplites. He discusses agriculture and includes a valiant attempt at a statistical argument. The last chapter examines economic and social tensions, and treats with welcome skepticism the hypothesis, now popular, that the rise of tyrants reflects a class struggle consequent upon the introduction of hoplite tactics.

The notes are relevant and erudite. Parallels from other societies (mostly from early modern Europe) are adduced frequently and with caution; often they serve, not to buttress a hypothesis, but to warn against a hasty conclusion. The discussion of Solon's work is careful and draws on extensive reading; yet in it one misses references to important new ideas expressed by A. Andrewes (*The Greeks* [1967]), J. R. Ellis and G. R. Stanton (*Phoenix*, 22 [1968]: 95-110), and M. Chambers (*California Studies in Classical Antiquity*, 6 [1973]: 1-16).

This is a judicious book; seductive simplifications are avoided; Starr is alive to the difficulties of conceptualization, and aware of the shortage of evidence. There are only occasional slips; for example, Thucydides (VI: 54) does not say that the archons in Pisistratid Athens had to be supporters of Pisistratus but only that some of them had to (p. 250). One may hope that the book will stimulate further inquiry into its subject, and one may guess that an additional line of approach to Greek economic and social growth might be the study of Athenian law in its historical development.

The eight plates are apposite to the argument. The publisher has furnished an elegant jacket, designed by Philip Bouwsma, and a generally handsome presentation, but has overlooked a few infelicities of language.

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W. E. HIGGINS. *Xenophon the Athenian: The Problem of the Individual and the Society of the Polis*. Albany: State University of New York Press. 1977. Pp. xv, 183. \$15.00.

It is the contention of William Edward Higgins that scholars of the nineteenth and twentieth centuries have not been fair in their assessment of Xenophon (though Higgins does openly profess his enormous debt to the work of Leo Strauss) and have been too predisposed to be influenced by his Peloponnesian and Spartan experiences to evaluate him correctly as one of the last great representatives of Athenian civilization. Higgins' book has a corrective function and attempts to study Xeno-

phon as a whole by examining *all* of his writings and by trying to deal directly with the Xenophonic texts.

Higgins' thesis focuses on Xenophon's understanding of the relation between the individual and the *polis*, not as the only unifying idea in Xenophon, but as an obviously crucial one that also relates to other issues such as family finance and interstate relations. In seven competent chapters the author deals challengingly with reading Xenophon, Socrates, Cyrus, tyranny, the active life, history, and Xenophon and Athens. The author's notes clearly show that his interest is in Xenophon's writings more than in other scholars' works on Xenophon. The reader will want to know that the current book is a thoroughly revised version of the author's Harvard doctoral dissertation and still bears some of the marks of its origin.

*Xenophon the Athenian* demands a close reading of Xenophon's texts. Xenophon is shown to be concerned with the relation of the individual to the city, but also with the relation of city to city, and the deeds of their leading men. In his own work Xenophon saw the Socratic effort to improve men as citizens. Improvement involves growth and the basic ingredient in proper growth is self-knowledge. Xenophon was certainly no revolutionary but tended to be conciliatory. He could not accept the thoroughgoing individualism of his time, but rather directed his intellectual effort toward "finding a way of integrating the excellence of the singular within the structure of the many, just as his style sought to achieve a harmonious unity out of a continuing respect for the value of the individual detail and the individual word" (p. 141). Xenophon is thoroughly aware of human inadequacy and the impermanence of human perfection. Success depends on an honest self-evaluation in terms of the actual power structure, but the excellent individual can provide the city with the right direction if the city will but see it.

Higgins provides the reader with a much-needed reassessment of Xenophon that goes beyond the usual study of military tactics, economics, and history. As a worthy student of Socrates, Xenophon is shown to be a mature thinker of considerable education and practical experience and an acute observer of his times. Higgins has produced a provocative book.

JOHN E. REXINE  
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JACQUELINE DE ROMILLY. *The Rise and Fall of States According to Greek Authors*. (Jerome Lectures, number 11.) Ann Arbor: University of Michigan Press. 1977. Pp. 100. \$12.50.

This book puts into print the four Jerome Lectures that Jacqueline de Romilly gave at the University of Michigan in September 1973. She examines the ideas of the major Greek writers who have written on the rise and fall of states and in her last chapter even finds space to say something about Roman writers, but her main focus is on the ideas of the great Greek historians Herodotus, Thucydides, and Polybius. Of these the most influential on the thought of de Romilly is Thucydides, as is justified both by the intrinsic importance of his ideas and the many contributions she has made over the years to our understanding of the greatest of ancient historians.

De Romilly asks whether the Greek view of the rise and fall of states was related to any general pattern or patterns the Greek writers saw in history as a whole and concludes that it was not. In her view the Greeks did not believe in a pattern of continuous progress or of continuous decline or in a cyclical theory of history. The use of the biological analogy, to be sure, is not uncommon, but it is only metaphorical and not the basis of prediction. The only pattern the Greeks applied to history was a characteristically Greek tragic one. They were not concerned with the reasons merely for the rise of states or for their fall, but for their rise and fall. The same reasons explained both phenomena, and they were human—psychological and moral. The study of the rise and fall of states, therefore, was not so much a scientific investigation as a moral problem: "Rise and fall are not only linked together in a sort of tragic contrast, illustrating the frailty of human condition and stirring up emotion, but are also connected with one another in a practical and threatening process, which at first stimulates intellectual curiosity and then, as the link becomes clearer, prompts a passionate effort to find a solution and teach it to mankind" (p. 18).

De Romilly's appreciation of the Greek outlook, derives chiefly from Thucydides; it is natural that men and states should seek power, and that having acquired some power they should seek to extend it. "As things go on, the path of prudence gets narrower and narrower: *hybris* and its dangers are rooted in the very nature of power" (p. 49). Some scholars will reject her view of Thucydides as a moralist, not rightly, I think, but few will fail to be interested by this learned and stimulating essay.

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FERGUS MILLAR. *The Emperor in the Roman World (31 BC-AD 337)*. London: Gerald Duckworth and Company. 1977. Pp. xvi, 657. £15.00.

If the study of history has relevance, the morphology of autocracy ought to head any list of topics for

investigation, since democratic societies are not the norm of the past, nor is their future prognosis bright. The most powerful and durable autocrats in Western history held sway at Rome—the Caesars and the popes—and Fergus Millar's splendid book deals with the former. Because the topic is large and spans four centuries, he has written a big book, full of evidence and exegesis, and has provided four brief appendixes on technical controversies. In assessing the role of the emperor in the Roman world the author steers a steady course between the Scylla of juristic formalism and the Charybdis of slipshod sociology. He knows that the Romans were not as tidy as modern authorities on Roman law, and he is careful not to contaminate his presentation of ancient evidence with generalities based on the experiences of other societies. Millar presents social scientists (who usually depend upon secondary sources) with a vast array of primary data, conveniently translated and sensibly explained. Moreover, he constantly reminds readers of what the evidence does *not* show. His sources include not only legal codes and historical anecdotes, but also the Gospels and philosophic novels, Galen and Epictetus, the dream book of Artemidorus, and other touchstones of antiquity neglected by narrower scholars. In different sections of the book, the author skillfully examines the same documents in various contexts, but the "highly selective" index is inadequate in recovering all of these valuable discussions. Many perceptive asides (for example, "the relatively static nature of wealth" [p. 158]) get lost in the sheer bulk of this volume.

Dismissing constitutional theory as a Barmecide feast, Millar goes to the heart of the matter and examines Roman autocracy in action. "The emperor was what the emperor did" (p. 6), and what he primarily did was respond to initiatives from below—inquiries, petitions, and proposals. The great value of this book lies in its emphasis on the passive nature of the imperial office. Vast distances and poor communications limited the effectiveness of despotism, benevolent or otherwise, and the primitive simplicity of governmental apparatus placed an enormous burden on the emperor as a man. The head of the Roman state had to devote his limited time and energies to a bewildering array of demands and impositions, which modern executives delegate to agencies and bureaucrats. In addition to his major concerns (military security, public order, and high policy), the emperor sat in judgment on trivial legal disputes and was beset by embassies from cities and associations and by requests from individuals for gifts of cash and goods and the award of privileges and exemptions from public duties. Not only aspiring politicians and buck-passing administrators, but also working people, unhappy wives, and ambitious academ-



ics—even the officers of “the Sacred Artistic Traveling Aurelian Oecumenical Great Synod” (p. 460)—demanded and received the emperor’s personal attention. In Ovid’s words, *res est publica Caesar* (*Tristia* 4.4.15). Originally, the imperial role was that of a provincial governor writ large and empire-wide, but his authority was never challenged by the fossilized remains of the Republic, and his subjects viewed the emperor as a Hellenistic *basileus*, whose response to their petitions had the force of law. As such, he was especially accessible to Greek rhetoricians who begged eloquently for favors for their cities, their friends, and themselves. The emperor’s staff was largely drawn from educated provincials from the East, an urban elite whose historical importance was greater than that of Claudius’ famous freedmen or the favorites of tyrants. Though he was expected to react promptly to foreign and domestic menaces (invasions, mutinies, famines, earthquakes, fires), “the initiation of change” was not “a normal and expected function” of the emperor (p. 271), and the innovations of the late empire were consistent with the role of a *basileus* under pressure. Even Constantine’s “revolutionary” adoption of Christianity as a favored religion followed patterns of relationship between an emperor and his subjects that had prevailed for centuries. While the generally passive role of the emperors in the sporadic persecutions of Christianity has long been recognized, Millar’s chapter on the Church and the Roman state is a masterpiece of sensible and sensitive scholarship.

The stated subject matter, the emperor in the Roman world, excludes the ruler as diplomat, and Millar avoids discussing the emperor as generalissimo, since a focus on military matters can obscure the civilian nature of the imperial office. He also passes over familiar examples of wastrels and incompetents who shirked the responsibilities of the throne, though he carefully credits feeble and erratic emperors when they put in a day’s work. His readers may forget that not every Caesar was at his desk before dawn or stayed awake during interminable audiences. Even the venerable Antoninus Pius liked to go fishing. Such cavils notwithstanding, this is a monumental work which will serve as a treasure trove of data and a necessary corrective. All students of Roman history and of autocracy are in Millar’s debt.

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HERMANN BENGTON. *Marcus Antonius: Triumvir und Herrscher des Orients*. Munich: C. H. Beck. 1977. Pp. 327. DM 29.80.

Hermann Bengtson has written a straightforward political and military biography of Mark Antony, concentrating—of necessity, given the available evidence—on the period from 44 B.C. onwards. The resulting narrative is lucid, smooth, and based on an obviously close familiarity with the sources. Errors of fact are infrequent, though some of them are disconcerting (Pompey founded numerous colonies in the East, p. 218). Especially good, as military narratives, are the accounts of the Mutina and Actium campaigns. The chapter on Actium contains an abundance of topographical detail and is probably the one most worth reading.

There is room for biographies of individuals as important and as enigmatic as Antony. But Bengtson’s book scarcely satisfies the standards that ought nowadays to be imposed on this genre of writing. One obvious requirement is a thorough treatment of the entire source problem, instead of a jejune appendix (pp. 299–305) supported by some scattered and not always consistent comments in the text. Very little written by Antony himself survives, or indeed was ever published. Both Cicero and Octavian did their brilliant best to blacken his character. Of the later sources, Plutarch’s life (interestingly it is almost the longest one he wrote) is a self-consciously dramatic production which presents a historian with manifold difficulties. So do the other major sources. It is a pity that Bengtson shrank from examining and explaining this issue in full.

Critical readers will also miss any determined effort to put Antony’s political and military activities in the contexts which alone reveal their significance. For instance, we naturally ask why Antony persevered with the Parthian war even when, in the years 34 and 33 B.C., self-interest might have suggested that he should concentrate his attention on the danger from Octavian. The question cannot be answered without reference to the developing traditions of Roman imperialism. And Bengtson unfortunately leaves us without any serious analysis of the shifting political loyalties of the period, in spite of all the help which the work of Syme, Brunt, and others have provided.

In short, a disappointing book. One of its most striking features, incidentally, is a pair of photographs illustrating a basalt portrait-bust of the subject. This is a very remarkable piece, and Bengtson might have told us (information which I owe to Professor Evelyn Harrison) that it was published by C. Vermeule and D. von Bothmer in *AJA*, 60 (1956): 330–31, its identity as a portrait of Antony having been suggested by B. Ashmole. This is worth knowing, because without the sculpture’s Egyptian provenance its antiquity might be doubted.

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STANLEY F. BONNER. *Education in Ancient Rome: From the Elder Cato to the Younger Pliny*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xii, 404. Cloth \$18.50, paper \$10.95.

This book "is concerned with human relationships, with perseverance amid difficulties, with the home as well as the school, and with those changes in society which affect both home and school" (p. xi). Its chief theme is the training of children and adolescents by tutors and at schools in the three Rs, in Latin and Greek grammar and literature, and in rhetoric, during the classical period of Roman history, from the mid-second century B.C. to the early second century A.D. Stanley Bonner laid some groundwork for this study in *Roman Declamation in the Late Republic and Early Empire* (1949). His new book will deservedly become the standard survey of Roman education.

The book is organized into three parts, "the historical background," "conditions of teaching," and "the standard teaching programme." The first deals with the traditional moral and practical training given to children (particularly boys) by their parents and other relations (particularly fathers, of whom the elder Cato was an unusually conscientious example). With the increase of Roman power and wealth, upper-class children were chiefly entrusted to slave "pedagogues" and tutors, and to professional teachers, many of them Greek. Bonner ably surveys the personnel, from the ex-slave Daphnis, who had changed hands at fees worthy of a modern footballer, or Tyrannio, an eminent professor captured in war and transplanted, not disadvantageously, to Rome, to the grumpy Italian, Orbilius the "Whacker," who taught Horace. All these were *grammatici*. Elementary education was given by more obscure individuals, including the slave child-minder, the *paedagogus* (or, for girls, sometimes a *paedagoga*, whom Bonner omits). Teachers of rhetoric are also scantily documented, partly because the well-born would-be orator learned much of his technique, particularly in the Republic, by living with a practicing advocate-politician and observing his methods and by going abroad to study under famous Greek academics at such centers as Athens or Rhodes.

Most of our information is on the education of boys and of the upper classes: this is reflected in the second part, where Bonner considers the poor accommodation of the humbler schools, the shortage of books and equipment, and the lack of a universal or state-financed system. Chapters 13 to 21, on the syllabus, are the kernel of the book, where Bonner's command of the ancient sources on education and scholarship produces a detailed account of the training available to the more fortu-

nate members of society—the sort of men who became prominent as politicians or as the classical writers of Latin literature. More space might perhaps have been given to philosophy; Bonner will not help those who want to know how Vitruvius got into architecture, what qualified a slave for the civil service, or what apprenticeship sufficed a hairdresser (the emphasis on literary education—which is, incidentally, what the civil service required—is entirely Roman); some would query the acceptance of the Romans' theory that Roman morals declined sharply in late Republic and early empire.

I make these reservations only in passing. Bonner keeps in mind the importance of the Roman system for the later history of education. This clear and delightfully readable book is one of the few which can be equally recommended to the specialist and to the Latinless and Greekless reader. The notes give the sources and compact but comprehensive references to modern work; the illustrations illustrate. Years of study, thought, and "the labour of the file" have gone to produce this authoritative survey, which, one might conjecture, attests to the efficacy of a classical education.

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#### MEDIEVAL

R. I. MOORE. *The Origins of European Dissent*. New York: St. Martin's Press. 1977. Pp. xi, 322. \$15.95.

Perhaps only an Oxford-trained scholar would dare to write a book that is not designed to fill any obvious gap, draw on any new sources, or construct any monolithic interpretation. Although this is a description of R. I. Moore's study, another is that it is impressive and important, as well as difficult and challenging. Heralding these last two traits is the title, unequipped as it is with any dates or subtitular elucidations. Readers who accept the challenge to learn what the book is really about will find that it deals with both less and more than the title suggests. Although Moore insists that he is writing about "dissent," a more conventional understanding would be that his subject is European popular heresy in the eleventh and twelfth centuries. Yet he also deals with more than that because he examines the social context of heresy far more extensively than is customary; indeed, Moore's book is often more a study of social dynamics than of doctrine.

To try to state its major arguments in a few sentences is like trying to do the same for Peter Brown's *Augustine*. (Moore, in fact, is a student of Brown's.) Most often Moore argues against neat explanations: eleventh-century heresy was dis-

parate in nature and untouched by Bogomil influence; most twelfth-century heretics were not simple "reformers"; heresy was not an expression of class warfare. Moore is untroubled by the fact that he offers no all-encompassing schemes to replace older ones, for he sees his heresies as differentiated and dappled things. Yet he does want to account for them as far as he thinks possible, and some outlines do emerge: eleventh-century heresy was provoked by dissatisfaction with the performance of the Church in a time of growing wealth and social tensions; twelfth-century heresy, until the coming of the Cathars, revolved around "charismatic" alternatives to Church leadership; the success of Catharism came primarily from its teaching that humans could conquer evil because it was external and from its provision of organized alternatives to the Roman priesthood.

To my mind, Moore's most original and stimulating chapter is that on Henry of Lausanne. Hitherto this twelfth-century heretic has been seen through the Grundmann prism as a preacher of reform and the apostolic life, and hence as a link between the Gregorians and Waldo, but Moore argues that Henry stood fully at odds with the reformers and held no brief at all with the Church. This is part of Moore's larger insight that the Gregorians and their twelfth-century successors wanted the Church to expand its competence over lay life while most heretics were hostile to priests because of their growing sacramental claims. It is in insights like these (and there are many others) that the book is at its best.

As Moore makes the reader think, he also sometimes makes him wonder. I am not happy with Moore's preference for the word "dissent" or with his conception of it (as I follow his logic he would have to make Joan of Arc a "dissenter"). I have my doubts too about several smaller points, and I think that Moore would have written a better book had he paid more attention to German scholarship. Perhaps the best way of summing up is to say that Moore has written an excellent work for connoisseurs. It cannot be recommended for undergraduates or even beginning graduate students because it takes much historical knowledge for granted and is thin on bibliography. Although Moore's prose is remarkably distinguished, it is also sometimes so sinuous as to require rereading. The rereading, however, is rewarding: those who have already started thinking about Moore's problems should find his book greatly stimulating and a thing of pied beauty.

ROBERT E. LERNER  
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TILMANN SCHMIDT. *Alexander II. (1061–1073) und die römische Reformgruppe seiner Zeit.* (Päpste und Papst-

tum, number 11.) Stuttgart: Anton Hiersemann. 1977. Pp. ix, 262. DM 135.

Alexander II enjoyed the longest reign of the eleventh-century reform popes before Gregory VII, and, by contrast with the activity both before and after it, one of the quietest. An investigation of Alexander II's association with proponents of reform, therefore, is to be welcomed. This revised Tübingen dissertation is a genuinely learned piece of research. Tilmann Schmidt has recognized the impossibility of writing a biography, and has further demurred from providing a full exposition of the pontificate. His expressed object is to examine whether Alexander II should be viewed only in the shadow of Hildebrand or if he may be perceived in somewhat sharper relief. As the treatment of the relationship of pope to cardinal is handled only in the last twenty pages, this statement is misleading. The book is in fact a study of a series of selected technical problems associated with Alexander II and certain of his contemporaries.

The work is organized into three parts of unequal length. After reviewing the pope's early life in Milan, the author casts doubt on the tradition that Alexander had studied at Bec with Lanfranc. He also argues persuasively that the claim that Alexander had been a chaplain at the imperial court rests upon a textual misunderstanding. In the second part Schmidt describes how, as bishop of Lucca, a post held in plurality during his pontificate, the future pope rendered unusual service to the papacy while becoming acquainted with Hildebrand and Peter Damian, his later supporters. The most valuable portion of the book, revealing fully the author's sensible, cautious judgment and flare for detail, is the first section of the third part concerning the election of 1061 and the subsequent schism. The analysis is thorough and balanced, although one may question whether Godfrey of Lorraine's influence has not been minimized. In the last section an effort is made to catalog all reforming clerics known to have been active in Rome during the third quarter of the eleventh century, and here historical analysis occasionally gives way to a species of prosopography. Some figures are fully described, others are barely mentioned. For example, the Dalmatian legation of Cardinal Mainard of Silva Candida, documenting papal extension of the reform program to the eastern Adriatic, has been overlooked.

In the concluding chapter Schmidt explores the evidence for the collaboration between Hildebrand and Alexander and notes the recorded instances of disagreement between them. Earlier in this century Z. N. Brooke expressed doubt about the conventional image of Hildebrand as the power behind Alexander's throne, observing that

Hildebrand "must often have chafed at papal inaction; it certainly disappeared at once when he became Pope." Schmidt's study provides the documentation to support Brooke's assessment.

Three excurses and a useful appendix discussing the now lost register of Alexander II are included in the work. An unsystematic check of the secondary literature listed in the final bibliography uncovered sixteen cited items, some highly significant, which were omitted.

JAMES ROSS SWEENEY  
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HERBERT GRUNDMANN. *Ausgewählte Aufsätze*. Volume 2, *Joachim von Fiore*. (Schriften der Monumenta Germaniae Historica, number 25, part 2.) Stuttgart: Anton Hiersemann. 1977. Pp. 456. DM 130.

This volume, the second of a three-volume collection of essays by Herbert Grundmann, contains six articles dealing with Joachim of Fiore and three more treating aspects of the Joachite tradition. Two other articles are so far removed from these themes that no space will be given to them in this review.

Over the years between the publication of his doctoral dissertation, *Studien über Joachim von Fiore* (1927) and the appearance of his last article on Joachim in 1969, Grundmann maintained a continuing interest in Joachim, for he saw Joachim as a "thinker" too often ignored in traditional treatments of thought and spirituality of the High Middle Ages. If Joachim was not, in form, an orthodox thinker nor one given to easily understood expression, he was no heretic, even though teachings attributed to him were formally condemned. When Thomas Aquinas rejected Joachim's teachings on the era of law under the Old Testament and the era of grace under the New, He was, according to Grundmann, working with a corrupt tradition, not the authentic writings of Joachim. In another article Grundmann examines Joachim's mode of biblical exegesis, by which he established his view of the proper relation between the freedom of the Church and the authority of the emperor. The Church, Joachim taught, should be a servant while the emperor should respect its internal freedom. Other articles concerned directly with Joachim offer numerous notes on biographical, bibliographical, and textual matters.

In his studies of the Joachite tradition, Grundmann fixes the Florentine authorship of a fourteenth-century illustrated book of papal prophecies, identifies the Minorite Alexander who composed a commentary on the Apocalypse in the middle of the thirteenth century, and edits the text

of *Liber de Flore*, a prophetic work written very early in the fourteenth century as a piece of spiritual Franciscan propaganda.

Grundmann interprets Joachim's place in Dante's *Paradise* (X-XII) in the company of Bonaventura and Aquinas and as a counterpart to Siger of Brabant as a sign of Dante's approval of the role of the prophet in the Church. Joachim was the prophet both in historical fact and as a type.

Although much of Grundmann's work was done some time ago, his scholarship was significant, and his writings are still important for students of Joachim and the tradition he inspired. This volume makes available articles originally scattered throughout journals that are often difficult to come by these days.

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Kent State University

C. M. D. CROWDER. *Unity, Heresy and Reform, 1378-1460: The Conciliar Response to the Great Schism*. (Documents of Medieval History, number 3.) New York: St. Martin's Press. 1977. Pp. 212. \$19.95.

The great councils of the late Middle Ages marked one of the most interesting phases of the Church's history. At no other time in the medieval period was the unity of the Church so seriously threatened, the challenge of heresy so pressing, and the cry for reform so intense. In addition to the ecclesiastical problems facing the councils, there were also important political factors that influenced their negotiations, such as the intervention of the rising national monarchies and the activity of the Holy Roman Emperor. In confronting the religious upheavals in Germany and Bohemia, moreover, the councils encountered the new economic and social forces that increasingly influenced religious matters in the late medieval period. Finally, the events of the Hundred Years' War did not fail to leave their impact upon conciliar history. All these developments are so clearly mirrored in the deliberations of the late medieval councils that the councils can be viewed as a microcosm not only of the late medieval Church but of the society and politics of that period as well.

This vast panorama of late medieval life as reflected in the councils of that time is now made available to us through C. M. D. Crowder's work, which is a collection of primary sources in translation. The documents chosen by the editor are all directly related to the major councils of the period: Pisa, Constance, and Basle-Ferrara-Florence. As is commonly known, the three major goals of the great councils were the restoration of ecclesiastical unity, the suppression of heresy, and the reformation of the Church. Crowder clearly made these



goals the principal norms in selecting the documents for his edition. Also included are documents which illustrate the political interests of the European monarchs—especially the English crown—in the conciliar deliberations.

As the editor himself admits, the selection of documents involves a certain degree of arbitrariness, and each reader will be somewhat disappointed that more prominence was not given his or her specific area of interest. I would have desired more emphasis on sermons and treatises related to ecclesiological and reform themes. Despite this minor reservation, it must be said that Crowder has succeeded admirably in selecting documents illustrative of the major concerns of the councils. Most of the thirty-five documents chosen are presented in new translations made by the editor himself. There is a very fine historical introduction to the whole collection as well as brief historical notes explaining the background of each document. Also provided is a comprehensive and up-to-date bibliography related to the entire conciliar period.

Crowder is indeed to be congratulated for making available to us an excellent and much-needed collection of late medieval conciliar sources. His work will be of great interest and value not only to students of the late medieval Church but also to everyone interested in the late Middle Ages.

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JAMES BUCHANAN GIVEN. *Society and Homicide in Thirteenth-Century England*. Stanford, Calif.: Stanford University Press. 1977. Pp. xiv, 262. \$12.50.

The core of this interesting and lively book is a quantitative study of all homicide cases appearing on twenty eyre rolls from the period 1202–76, preserved in the Public Record Office in London. The cases come from five English counties—Bedford, Kent, Norfolk, Oxford, and Warwick—and from two urban areas—Bristol and London. The author analyzes the cases in terms of relative frequency of homicide, in terms of the status, sex, and family relationships of killer and victim, and in terms of the nature and final disposition of each case. He presents the results in twenty-three tables, illustrates them with examples drawn both from record and literary sources, and suggests explanations and interpretations for his figures. He also draws comparisons from modern crime statistics to highlight the special characteristics of thirteenth-century homicide.

James Buchanan Given's discoveries are diverse and extensive, too much so for adequate summary. Some are expected, like the finding that murder was frequent or that it was overwhelmingly a

crime committed by men against men. Some are surprising, like the finding that murder occurred more frequently in the country than in the city. Among the most interesting is the discovery that almost all the cases involved killing by the humble ranks of society, not by the gentry or nobility. From this follows the conclusion that "violence was a tool resorted to by the poor and the weak" (p. 90) and that the behavior of the English upper classes was "very different from that of their counterparts in the rest of Europe" (p. 74).

The assumption underlying the book's analysis is that the eyre records can be taken at face value. Given has been obliged to accept that all those accused of murder had in fact killed someone and that the records are relatively complete. It is difficult to see what else he could have done without undercutting the presentation of his discoveries, but perhaps he might have said more about the possibility that the persistence of habits of private settlement in criminal matters, the fear of possible amercement against presenting jurors if they did not indict *someone* for a death, and the use of criminal law for purposes of harassment may have skewed his figures. Thus men of substance may figure so rarely among the accused because they were normally subject to a criminal appeal, brought by the victim's kin in hopes of securing a settlement, which never appeared on the official eyre roll. (See J. M. Kaye, *Criminal Law Review*, [1977]: 4–13.) Given admits to speculation, however, and the core of his book—the quantitative analysis of the eyre roll cases—together with a readable presentation of the results, make this a valuable addition. It takes murder out of court and puts it into a social setting.

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Washington University

R. HOWARD BLOCH. *Medieval French Literature and Law*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xii, 267. \$14.50.

The thesis of this stimulating book is clear and simple: just as a shift occurred in France between 1100 and 1250 from the ordeal by battle to the written inquest, that, in turn, exemplified broader shifts from warfare to legal machinery for deciding disputes, and from baronial domination to centralized monarchy, so parallel shifts occurred in contemporary French vernacular literature that reflected these legal changes. The first phase of R. Howard Bloch's argument succeeds well. The crisis of the ordeal around 1200 was indeed expressed in vernacular literature. While scholars have long noticed the prevalence of ordeals in medieval literature, Bloch shows from a discussion

of two particular cases involving Lancelot and Guinevere in the *La mort le roi Artur* (from which he draws his principal materials) how doubts were raised over the efficacy of ordeals. Moreover, questions were raised about the wisdom of deciding disputes by warfare. The portrayal of battles as chaotic, of sieges as protracted and ineffectual, and the interminable debates over the pros and cons of campaigns, all indicated a general weariness over warfare in baronial circles. In *La mort* the aristocratic vendetta was depicted as destructive of an entire kingdom.

Historians will find the second phase of Bloch's argument less convincing. His treatment of the appearance of the inquest is too vague to indicate its important developments. Since, with one possible exception, no inquests appear in vernacular literature, Bloch is obliged to modify his comparative approach. At this point he distills three general principles from the inquests (individuation, abstraction, and verbalization) and applies them to the lyric poetry and romances of courtly literature. But these principles are themselves too abstract and broad to be of much use. When the incident of King Marc's hesitation to kill the adulterous Tristan and Iseult caught *in flagrante delicto* is used to demonstrate the sublimation of physical vengeance and therefore the origins of the modern state, historians may demur.

In general historians will not be satisfied with Bloch's treatment of historical subjects per se, which relies on a too limited bibliography and contains a fair number of factual errors. In particular they might feel that the role of the Church has been underestimated in accounting for the disappearance of the ordeal and the rise of the inquest. But historians should applaud and encourage Bloch's efforts to reimplant vernacular literature in its historical context. After a stultifying season of "self-referential criticism," literary scholars like Bloch are beginning to break down the confinement of the text, and to seek light from the social milieu. Bloch's book is an important step in this direction that should stimulate both literary scholars and historians to more fruitful cooperation.

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MARIO DEL TREPPO and ALFONSO LEONE. *Amalfi medioevale*. (Biblioteca di Studi Meridionali, number 5.) Naples: Giannini Editore. 1977. Pp. xii, 340. L. 12,000.

The two separate monographs which make up this study of the economy and society of medieval Amalfi in the period of the ninth through the fif-

teenth centuries complement one another with a coherence not always typical of such cooperative ventures. Informing the work as a whole is the problem of when, how, and to what degree Amalfi went into economic eclipse within the Mediterranean commercial world. But, as Mario Del Treppo points out in his essay, "Amalfi: Una città del Mezzogiorno nei secoli IX-XIV," in order to understand Amalfi's near legendary decline with any historical precision, it is necessary to establish a kind of base index of prosperity against which to measure the degree and scope of the fall.

Thus Del Treppo undertakes to analyze the structure of Amalfitan society and the dynamic of the social and economic changes that took place within the chronological limits he has set for himself. Del Treppo characterizes the period prior to the Norman conquest of the South as one of decided economic prosperity resulting from the favorable "conjuncture" of proximity to the Arab world, close ties with Byzantium, and a lack of commercial competition. He argues, however, that the twelfth and thirteenth centuries did not mark a time of decline. Rather, he describes Amalfi in the central Middle Ages as a city of moderate economic well-being, dominated by an old aristocracy which controlled agriculture, commerce, and the Amalfitan Church. This nobility was based upon a few old families; Del Treppo convincingly demonstrates that what appear in the sources to have been new families rising into the aristocracy were in fact offshoots of the old patriarchal lineages. Amalfi's disappearance from the international commercial scene was ushered in by the war between the Angevins and the Aragonese which disrupted Southern Italy and Sicily in the last decades of the thirteenth century. The fourteenth century for Amalfi was one of unrelieved calamity in the form of famine, plague, depopulation, and brigandage, as well as a tidal wave that destroyed much of its port facilities in 1343. From all of this it failed to recover.

Alfonso Leone, in his "Amalfi e il suo commercio nel secolo XV," provides an anchor, as it were, to Del Treppo's work by determining the level of *quattrocento* Amalfitan commercial and economic activity. He clearly shows that Amalfi's commerce in the fifteenth century was restricted to trading with its neighbors of the southern *Regno*. Contacts with the larger Mediterranean commercial world took place through the mediation of foreign—primarily Genoese—merchants, who imported the goods of international trade such as Turkish cotton into Amalfi. Leone also considers the structure of society and concludes, on the basis of a judicious study of the surviving notarial materials, that the nobility continued to dominate. If anything, the aristocracy was drawn more closely



together, constituting a virtually closed class in the fifteenth century. It did so not because of a challenge from new rising social groups but rather because by closing ranks its members were better able to defend themselves in a time of economic stagnation.

These brief comments inadequately convey the richness and originality of this work. It is an important study that will be of interest not only to historians of Italy's south but to all scholars concerned with the medieval urban scene.

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FRIEDRICH LOTTER. *Die Konzeption des Wendenkreuzzugs: Ideengeschichtliche, kirchenrechtliche und historisch-politische Voraussetzungen der Missionierung von Elb- und Ostseeslawen um die Mitte des 12. Jahrhunderts*. (Vorträge und Forschungen, Sonderband 23). Sigmaringen: Jan Thorbecke Verlag. 1977. Pp. 92. DM 34.

This slim volume—eighty pages of text, each page greatly shortened by citations—investigates the concept that the Wendish Crusade of 1147 was conducted on the basis of “baptism or death.” First the author follows this slogan from Bernard of Clairvaux through the commentaries of churchmen and through popular movements to a conclusion that, whereas Bernard meant only the death of the Slavic state, not the massacre of the population of that state, the mass of crusaders interpreted it much more literally and were as ready to apply it to the Abodrite Slavs as to the Jews. Second, in reviewing the previous German attempts to conquer the Slavs along the Elbe, he concludes that by 1147 the Abodrites had only the alternatives of being conquered—and thereby being submerged politically and culturally—or becoming a Christian state within the Church and the Empire. This was the choice offered by Bernard. In choosing Christianization, the Abodrite leaders made possible an end to the ancient hostility between German and Slav in Mecklenburg and Pomerania.

The book was written for the specialist who is already familiar with the literature and the history of the German eastward migration, and secondarily for those interested in the thought of St. Bernard. For the nonspecialist Friedrich Lotter offers only a readable style, a concise organization, a thorough bibliography, and some small insight into the minds of twelfth-century German scholars, nobles, and common men. The volume is a typical German monograph, aimed at a handful of scholars, thoroughly footnoted, and issued in an unattractive paperback format. Consistent with this, there is not even a map to assist the non-expert and the introduction is perfunctory.

The strength of the book lies in the author's singleminded concentration on the two themes. He refuses to be distracted long by the competing political ambitions of the principal figures of the era, the process of German immigration, or the controversies of the *Drang nach dem Osten*. The slogan “*Tod oder Taufe*” was not humanitarian, he emphasizes, but it did succeed in ending long and bloody warfare, in controlling both the ambitions of the nobles and the passions of the mob, and therefore in saving the Abodrite state from subjugation and destruction. If Christianization was not achieved immediately, at least that country was opened to an ultimately successful missionary activity.

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A. P. KAZHDAN. *Armiane v sostave gosподstviushchego klassa Vizantiiskoi imperii v XI-XII vv.* [Armenians in the Ruling Class of the Byzantine Empire in the Eleventh and Twelfth Centuries]. Erevan: Izdatel'stvo AN Armianskoi SSR. 1975. Pp. 190. 1 r. 48 k.

A. P. Kazhdan's research on the role of the Armenian component in the ruling class of the Byzantine Empire during the eleventh and twelfth centuries, now published as a separate volume, was originally intended as a preliminary study to be included in the author's more general work, *Sotsial'nyi sostav gosподstviushchego klassa Vizantii XI-XII vv.* [The Social Composition of the Ruling Class in Byzantium, Eleventh and Twelfth Centuries] (1974). Despite this transformation, the present work stands satisfactorily in its own right as an interesting case study of a particular group within the imperial ruling class at a significant time for both parties. In Byzantium, the late eleventh century marked the watershed between the traditional, bureaucratic, service aristocracy and the new, hereditary, military, landed nobility coming to the fore with the accession of the Comnenian dynasty. For the Armenians, this period of hiatus between the destruction of the Bagratid kingdoms of Greater Armenia by the Seljuks and the re-creation of an Armenian state in Cilicia meant a massive migration of Armenian magnates into imperial territory. Hence, a consideration of this period which Kazhdan sees as the apex of Armenian participation in Byzantine administration is especially valuable.

Taking the studies of Nicholas Adontz and Peter Charanis as a partial point of departure, A. P. Kazhdan has gone on to develop and refine the results of these earlier works. Instead of investigating particular clans or tracing the role of Armenians in general, Kazhdan has concentrated on the

eleventh and twelfth centuries and attempted to bring greater precision to the identification of certain aristocratic families as "Armenian." In doing this he has not only subdivided the thirty-eight families of the period having possible Armenian antecedents into three groups (unquestionable Armenians, Ibero-Armenians, and possible but unproven Armenians), but also provided us with a complete prosopography including all identifiable members of these families. To this he has finally added a survey of isolated Armenian leaders surfacing for one generation only, as well as new and useful classifications according to chronological or geographical distribution.

The quantitative analysis by which Kazhdan postulates that the Armenians formed ten to fifteen percent of the ruling class of the empire in the eleventh and twelfth centuries, that about forty percent were great landowners, or that only seven percent were civilian as against military administrators in the eleventh century must remain questionable, as the author is the first to note, because of the fragmentary nature of the data and especially the absence of explicit evidence for the Armenian background of his doubtful third group, which includes such active and powerful families as the Phokas, the Skleroi, and the Dalassenoi. There is no reason, however, to challenge his broader conclusions that the Armenians came to power by way of the army especially in the eleventh century, that their sphere of activity lay along the borders rather than in the core of the empire, or, most interestingly of all, that their careers show a decline in the twelfth century as they slipped from the dominant military aristocracy into the class of civilian administrators and clerics. Equally important is Kazhdan's moderation of the earlier enthusiasm for identifying all hypothetical cases as true Armenians and his valuable caveat that purity of blood was not a criterion for inclusion into the ecumenical imperial aristocracy, so that a narrowly ethnic approach may often be misleading. This is especially the case for some Armenian converts to Chalcedonian Orthodoxy who had been in the service of Byzantium for several generations and who may not have been unduly conscious of their separate ethnic or cultural identity. Consequently, Kazhdan wisely chooses to use the mitigated terms Byzantine-Armenians, or in some cases Ibero-Armenians, rather than attempt a more rigorous but ultimately distorting precision.

Many loose ends perforce remain in view of the scattered and unsatisfactory nature of the sources. Some further questions might have been raised, such as the way in which Armenians, a number of whom remained Monophysites, on Kazhdan's own showing, while others were avowed Paulicians in Italy and the Balkans, could have reached high

rank, especially in the army, when an oath of Orthodoxy was presumably required to enter the imperial service. Nevertheless, A. P. Kazhdan's careful and sensitive sifting of the available evidence, epigraphic and sphragistic as well as narrative, Armenian as well as Greek, and his masterly familiarity with Byzantine society have greatly advanced our knowledge of the subject to which he has addressed himself here. He has provided a useful model for similar studies.

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## MODERN EUROPE

FERNAND BRAUDEL. *Afterthoughts on Material Civilization and Capitalism*. Translated by PATRICIA M. RANUM. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1977. Pp. xi, 120. \$7.95.

Retrospection upon future events is an unusual and challenging genre, unusual since it frees the author from the ordinary constraints of historical writing, and challenging both in the resultant opportunity to stress structures and theories unencumbered by documentation and in the explicit confidence demanded from the reader. In this concise book—the published version of a lecture series delivered at Johns Hopkins in 1974—Braudel summarizes the broad themes of his three-volume *Civilisation matérielle et capitalisme, 1400–1800* and offers his reflections on the historian's craft and on the nature of the historical imagination. Since only the first volume of *Civilisation* is yet in print, *Afterthoughts* is in reality a preview that serves to stimulate impatient anticipation of the publication of the two remaining volumes.

The book is organized in three chapters, each presumably reflecting the contents of a volume of the larger work. The first touches and sets in motion the themes of the already familiar *Capitalism and Material Life*, the most basic drives and efforts of economic man. The second chapter is devoted to "those exchange relationships" which Braudel calls "both the market economy and capitalism." For him, the transparent market economy has long existed and is characterized by the tradesman, *mercante a taglio*, or *Krämer* as opposed to the merchant, *negoziante*, or *Kaufherr*. The latter group's small sector, free of parochial political control and graced by enormous profits, was the preserve of capitalism. Immune from domination by the state, yet requiring its complicity, capitalism could develop and exploit political hierarchies already in place. In the final chapter, Braudel offers to "work the puzzle," "to establish the connections between

capitalism, its development and modes of action, and a general history of the world." Here one moves from city-empires to national markets and national economies. It is in the latter realm that one finds modern capitalism, a phenomenon whose origin and existence are explained by both internal and external factors. For Braudel, capitalism was and still is based on the exploitation of international resources and opportunities and it continues to rely on legal or de facto monopolies. Perhaps more strikingly, he notes that capitalism does not overwhelm all else and that it remains necessary even today to divide the economic world into three sectors—material life, the market economy, and the capitalist economy—if our view is to be comprehensive. Failure to make this distinction accounts for the misperceptions of politicians and economists of the modern West and the USSR of their own and of their opponents' economic systems and inherent politico-economic debilities. The book ends with a small coda disavowing the permanence of any historical work and urging younger historians to collect profit rates, production curves, and balance sheets and to attempt to construct national income accounts for the past.

Taken as a whole, the book is provocative and stimulating. On occasion, it rises to revelation when two or three sentences of compressed but brilliant prose force us to reconsider the events of an entire century or the history of a continent. Often, however, at precisely the same moment, the reader most regrets the absence of supporting documentation; it is for this reason that I mentioned the explicit confidence which the author demands. When the balance is struck, however, who more than Braudel has shown his willingness to provide detailed evidence for global history and who has done more to merit our confidence.

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FRANKLIN L. BAUMER. *Modern European Thought: Continuity and Change in Ideas, 1600–1950*. New York: Macmillan. 1977. Pp. xv, 541. \$14.95.

The central theme of this admirable synthesis of modern European intellectual history is mankind's ever-increasing sense of becoming, or process and flux, as opposed to the sense of being, or the static and the absolute. The subtitle prepares the reader not only for the change, but for its gradualness, and for the continuing presence of both approaches and the enduring tensions between them, at least until our own century. Franklin L. Baumer has identified five "perennial questions" which, with varying emphases, the modern centuries have

faced—the questions of God, nature, man, society, and history—and has organized his work around them rather than the contributions of individuals.

In seventeenth-century thought, the author asserts, being remained, on the whole, triumphant over becoming, despite notable readjustments in the scientific picture. Deity was immutable, human reason was generally thought to be in touch with ideal absolutes, and historical progress was a concept of very limited applicability. The eighteenth century still found universal standards alluring but was coming to emphasize man and his progressive potentialities, as contrasted with the predictable uniformities of nature. With many hesitations during that century, development and perfectibility in the historical record were increasingly noted. In the nineteenth century, the sense of becoming was at last dominant over that of being. In this, the longest of the work's major sections, the author introduces a useful four-part division of leading intellectual styles—Romanticism, the "New Enlightenment," the evolutionary world, and the complex *fin-de-siècle* mood. The first half of the twentieth century saw the almost undisputed triumph of becoming, the virtual eclipse of being, and the reign of relativism and despair. Restoration of a healthy tension between being and becoming, he concludes, is now vital to mankind.

Baumer's success in communicating an awareness of complexity in European intellectual life is remarkable in a book so compact and comprehensive. To illustrate the point, one may cite his cautious analysis of eighteenth-century ambiguities concerning human nature and the questions of historical progress, degeneration, and flux, or his appreciation for the rich subtleties of the Romantic movement. Of course, to cover so much, so successfully, the author has had to make a number of difficult choices, and not all his choices will satisfy all his readers. He cheerfully admits, for example, that his coverage extends only rarely beyond the English-French-German orbit. He not only rejects traditional organizational schemes according to individual thinkers or academic disciplines, but also rejects a sociological approach that replaces great ideas by popular thought as the main substance of intellectual history. This reviewer finds Baumer's methodology appropriate to the scope of the work; perhaps a few readers will find it confusing or old-fashioned.

Similarly, dissatisfaction is possible with the author's attempted integration of the visual arts with verbalized expression. There are forty-three illustrations, unhackneyed in choice, adequately reproduced in black and white, and placed close beside the relevant references. The references are generally too brief to be very informative on the art itself, but they do illustrate the interdisciplinary

connections within world-views—a gratifying step forward in a work of professedly intellectual rather than broadly cultural history. Finally, one should applaud the useful indexes (of proper names and of subjects) and regret the very brief bibliography—which, however, is supplemented by detailed, partly bibliographical footnotes. All in all, Baumer has written a perceptive guide to the main directions and many of the intricacies of modern European thought. It deserves to become a classic of twentieth-century intellectual history.

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E. E. RICH and C. H. WILSON, editors. *The Cambridge Economic History of Europe*. Volume 5, *The Economic Organization of Early Modern Europe*. New York: Cambridge University Press. 1977. Pp. viii, 749. \$38.50.

This long-awaited volume is the second of the series to be devoted to the early modern period. While its predecessor focused on what the editors call “the formative influences” which shaped the European economy, the present volume deals with the ways the economy responded to those influences, and notably with the production of goods and services.

In an introductory chapter one of the editors, C. H. Wilson, has provided a stimulating survey of the major trends in the economic historiography of the past fifty years as well as an admirably balanced assessment of its achievements and limitations. Agriculture is the subject of the next chapter by B. H. Slicher van Bath, the Dutch historian who over the years has contributed so much to the field of agrarian history. His approach combines the study of the technical aspects of agriculture proper with an analysis of rural society; accordingly, a great deal of attention is devoted to demographic and price trends, changing terms of trade between farm and town, wages, rents, and social conditions. In the last two sections of the chapter the author turns to such controversial issues as the rise of serfdom in Central and Eastern Europe and social stratification in Europe at large, and in both instances offers new and challenging interpretations. Chapter three by A. R. Michell deals with the great fisheries. Students of the period will applaud the editors’ decision to include a separate chapter on this subject and will feel indebted to its author for providing both a clear synthesis of the existing literature on this important, yet generally neglected, primary industry and much new evidence on its size and linkages to the rest of the

economy. In chapter four Kristof Glamann discusses trade. Departing from the traditional emphasis on luxury and colonial trades, he has devoted nearly half of his essay to those less glamorous but vitally important trades involving foodstuffs, nonprecious metals, and ordinary textiles. In his analysis of commercial organization, moreover, he has clarified a number of significant points, notably the nature and extent of monopolistic practices in early modern times.

The intricacies and complexities of the monetary and credit systems which kept trade flowing are the subject of a chapter by Herman van der Wee. His is possibly the most comprehensive discussion of the subject now available. Although many a reader will find van der Wee’s terminology too technical and indeed esoteric at times, few will fail to appreciate the scope and thoroughness of his essay, his ability to spotlight and evaluate financial innovations, and his discussion of the role played by government indebtedness in the development of modern financial institutions. In chapter five Barry Supple brilliantly investigates “the nature of enterprise.” In dealing with this elusive topic, he takes a critical, but consistently balanced, look at such controversial issues as the Protestant ethic, the merchant’s rationality, the social origins of preindustrial entrepreneurs, and their aspiration to become landowners; throughout, he stresses the historical context in which entrepreneurs had to act—a context of appalling risks, imperfect market structures, and precarious communications. The chapter on industry is the work of Hermann Kellenbenz. Based as it is on a prodigious amount of information gathered from publications in nine different languages, it provides a truly global view of the secondary sector of the European economy from the sixteenth to the eighteenth century. And Kellenbenz’s analysis of mining and metallurgy will be especially welcome to students of the period who lack either the time or the linguistic skills needed to venture into a field in which Central European countries loomed so large. The final chapter, by Betty Behrens, is a lucid, incisive portrayal of the social and institutional context in which economic activity took place; it is also a masterly analysis of the slow, fitful, yet unmistakable transition from a corporate, hierarchical society to one based on bourgeois individualism.

Far more space would be needed to do justice to a book of this size, scope, and distinction. But it is only fair to say that what we have here is an impressive achievement, a rich distillation of mature scholarship, and an indispensable guide to the study of the early modern economy and society.

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PETER KRIEDTE *et al.* *Industrialisierung vor der Industrialisierung: Gewerbliche Warenproduktion auf dem Land in der Formationsperiode des Kapitalismus.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 53.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 393. DM 56.

This book is a study of "proto-industrialization," which Peter Kriedte, Hans Medick, and Jürgen Schlumbohm define as "the evolution of rural regions in which a large part of the population supported itself completely or to a considerable extent through industrial mass production for supra-regional and international markets." Using a "combined theoretical-narrative form," the authors synthesize a huge amount of recent work on the decline of feudalism, on rural industry, demography, the family, and early capitalism. Their elaborate description of proto-industrialization is intended as a conceptual framework for future monographs. They also reprint excellent case studies by two of the leading scholars in the field, Franklin F. Mendels and Herbert Kisch.

One cannot summarize a book of this scope in a brief review, so a few comments must suffice. Although the authors do not address the issue directly, their argument contradicts the old saw that cities helped to undermine feudalism by providing alternative employment for peasants. In fact, industry began to emigrate to the land in the later Middle Ages because guild monopolies could no longer satisfy the demand merchants had discovered. Rural industry was thus the real alternative to farming, but until perhaps the seventeenth century it was no more than a secondary occupation for peasants with too little or marginally productive land. Hans Medick's discussion of the interaction of rural industry, demographic trends, and family structure is especially interesting, because most of the work in this area is so recent. He shows that small producers were trapped in poverty. Forced into debt by slack demand, families often compensated by producing more, thereby depressing prices still further. As agriculture was slowly abandoned in favor of industry, social patterns changed. Patriarchal authority weakened, since the land controlled by parents was no longer the only source of subsistence. The average marriage age declined sharply, and couples had children rapidly to augment the family work force. Despite infant and childhood mortality much more severe than that of agrarian households, high birth rates steadily increased the population and thus the supply of cheap labor. It was precisely this cycle of misery which made rural products competitive in distant markets and profitable for middlemen.

Like many collaborative efforts, this one has shortcomings. Kriedte and Medick argue that

proto-industrialization was not just the beginning of industrialization, but a distinct historical period, while Schlumbohm contends that it was transitional and requires different models for its various stages. The authors apparently agreed to play down these differences, but each of them tactfully rephrases from his own point of view much of what the others say. The individual chapters are thus comprehensive, but the book as a whole is repetitive. Kriedte and Medick's sociological style is somewhat pretentious, but it rewards careful reading. The authors cite a vast array of literature from German, English, French, Polish, Italian, and Dutch sources, but fail to provide a bibliography—most annoying. Nonetheless, this is an enormously valuable book; the centuries since the Middle Ages will never look quite the same.

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LOUIS J. LEKAI. *The Cistercians: Ideals and Reality.* Kent, Ohio: Kent State University Press. 1977. Pp. x, 524. \$20.00.

This broad synthesis traces the history of the Cistercian Order from its beginnings in 1098 down to the period after the Second Vatican Council. Part one provides a chronological account of Cistercian history; part two treats the salient aspects of Cistercian culture: liturgy, spirituality and learning, economy, architecture and art (strengthened by two sections of illustrations), lay brotherhood, and the nunneries. Appendix one offers an excellent translation by Bede Lackner of the most important early Cistercian documents, and appendix two contains statistics on the personnel of Cistercian houses from the early sixteenth century to 1974. The twelve historical maps are accurately drawn and easily readable. The invaluable bibliographical essay incorporates the rich literature on the Cistercians published since the appearance of Louis J. Lekai's *The White Monks* (1953). *The Cistercians: Ideals and Reality* will become the standard reference work for all students of Cistercian history.

For the medieval centuries the focus is predictably on France, the location of Cîteaux and Clairvaux, but Central and Eastern Europe, Italy, the Iberian peninsula, and England receive balanced attention. For the more recent period, one is struck by the survival of Cistercian monasticism in Yugoslavia and Poland, and its growth in North and South America, Africa, and in such Asian lands as Japan and, as of 1974, Vietnam.

The contribution of this study, then, rests on its beautiful assimilation of a huge literature, the understanding which Lekai brings to the subject, and

on his felicitous and sometimes quietly ironical style. The sections on the eleventh-century origins of Cîteaux with its careful chronology of developments, on the querulous and pretentious de Rancé, on the conflict within the order between the proponents of the Strict and those of the Common Observance, and on post-Vatican II monasticism are among the most significant.

Lekai's interpretation is traditional: the great twelfth-century Cistercian expansion is ascribed primarily to the moral influence and personality of St. Bernard and to the special appeal of Cistercian spirituality to intellectuals of the period; decline in the fourteenth and fifteenth centuries resulted from the failure of Cistercian monasticism to respond to the needs of a changing society. Again and again, the author shows that the ideals of the Cistercian founders were too exalted for practical reality. The rules and laws of the General Chapters frequently had to yield to the realities of local economic and social conditions.

In striking contrast to its pervasive theme, the book's structure treats Cistercian monasticism as if a sharp dichotomy existed between it and the broader society of which it was a part. Thus, the last chapter, "Monks and Society" is largely given over to the *services* the monasteries performed for the lay world. Since Lekai acknowledges that "their destinies were shaped by the society in which they were born" (p. 93), since monasteries could not come into existence, grow in numbers, power, and wealth without the support of the broader society, a different organization would have better pleased the historian of medieval society.

This caveat aside, this book will take its place beside the works of David Knowles and Philibert Schmitz as fundamental for the study of monastic history.

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ROLAND H. BAINTON. *Women of the Reformation: From Spain to Scandinavia*. Minneapolis: Augsburg Publishing House. 1977. Pp. 240. \$9.95.

*Women of the Reformation: From Spain to Scandinavia* is Roland H. Bainton's third volume in a series on "women of the Reformation." While the first dealt exclusively with German- and Italian-speaking women and the second with French- and English-speaking ones, this volume covers a much greater variety of cultures and countries: Spain, Portugal, Scotland, England again, Denmark, Norway, Poland, Sweden (in that order), and Hungary and Transylvania. All in all twenty-eight women are

dealt with, some in detail and depth, and some very briefly. Spain and Poland receive the lion's share (about fifty pages each), and the most interesting presentations. The Kingdom of Bohemia, with its Hussite and Bohemian Brethren movements, is passed over in silence.

The great range which the author covers in this volume adds interest to the book and at the same time weakens it, since too much is attempted in too little space. The spread enables the reader to realize the interrelations of European society at the time of the Reformation, one of the author's aims. But the wide range of his reports proves a little bewildering, and also makes quite a few of the sketches too "sketchy."

Most of the "women of the Reformation" are Protestant or tainted with other heresy (in one case, witchcraft). A few are examples of sixteenth-century women who managed to maintain their internal and external bond with Roman Catholicism—St. Theresa of Avila and Anna Marchocka Teresa, her Polish admirer, in particular. Some of the figures receive ample biographical treatment and characterization; others are brought in only to show a particular role or contribution or trend. Many are clearly religiously involved in the sixteenth-century reform movement; others simply happen to be living at the time, and exhibit only secular, especially political, interests (like Bona Sforza, an Italian queen of Poland, and Isabella Zapolya, Queen of Transylvania). We are given very little information as to the religious roles and attitudes, or even personalities, of some. A major reason for the absence of information about them as persons is often simply the lack of such information in the sources. The author presents several of the women simply through the medium of the men with whom they were associated (e.g., the Queens Dorothea and Anna of Denmark are shown through their husbands, Christian III and Frederick). The meagerness of extant sources written by women themselves is a common problem in women's studies.

Another difficulty Bainton faced, when covering Nordic or East European countries, was the problem of languages. He acknowledges his indebtedness to those who helped him with the language problems involved, but the problem still limited his research. The author has enriched the English-speaking audience by venturing into areas usually neglected by "Western" historians (what could be more western in Europe than Portugal, though?), but his aim may have been too broad. However, his presentations tantalize and whet the appetite for further research in women's studies in countries usually neglected by Anglo-American students of history.

One might wish that he had done more by way



of conclusions and comparisons. The work as a whole lacks cohesion. The study would also have profited had Bainton not tried to cover so many figures, but concentrated instead on persons who were clearly "of the Reformation," and analyzed them more fully. The desire to be culturally and geographically inclusive can only be welcomed and encouraged. It naturally presents problems for the scholar trained in Central and Western European history.

One conclusion to be drawn from the variety of figures presented is clear: women of all social strata in the sixteenth century actively participated in vital aspects of the Reformation in many and different countries, and were as heroic (for example, during the Inquisition) and independent-minded as the men of the era—although they did not write or teach academic theology and were not members of the clergy. This seems to be the unstated but well documented thesis of the book. It gives modern historians a needed perspective: the history of religious movements is not necessarily formed just by professionals, and therefore not just by men.

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JOHN H. LANGBEIN. *Torture and the Law of Proof: Europe and England in the Ancien Régime*. Chicago: University of Chicago Press. 1977. Pp. x, 229. \$16.50.

This is a short, tightly written book that dispassionately deals with a distasteful subject. There can be few less pleasant human activities than torture, especially when it is inflicted by the state as part of its judicial procedures, or to protect national security.

On the Continent torture evolved from the medieval ordeal. It became a necessary part of the judicial procedures for serious crimes, when Roman law demanded a high level of proof to convict—usually two witnesses or a confession. Because such were often not available, suspects against whom there was a considerable amount of evidence, amounting to "a half proof," could be tortured, not to make them plead guilty, but rather to confirm or deny their guilt by revealing details about the crime that only the perpetrator could know.

According to the widely accepted view torture was abolished in the eighteenth century in Europe largely thanks to the influence of enlightened writers such as Voltaire or Cesare Beccaria. John H. Langbein disagrees with this "fairytale," convincingly showing that torture ended when it was

no longer needed. Since in the Middle Ages death was the only means of punishing serious criminals, a high degree of proof was necessary to prevent mistakes—posthumous pardons and apologies being of little consolation. In the sixteenth and seventeenth centuries as the state developed less final punishments, such as the galleys, the workhouse, or transportation, the level of proof required decreased, and so did the need for judicial torture.

The justification for torture in England was very different from that on the Continent. Torture never became a regular part of the English judicial procedure since a jury could convict a man on less evidence than a Roman law magistrate. In England only the Privy Council or king could authorize torture, and of the eighty-one cases from 1540 to 1640, three-quarters dealt with state crimes, such as the Martin Marprelate investigation, the Guy Fawkes conspiracy, or the Catholic missionaries Henry Garnet and Edmund Campion. As Blackstone noted, in England the rack was "an engine of state not of law." In this regard the English use of torture to discover facts and accomplices was surprisingly modern, being similar to that (for example) of Chile, South Africa, or even Israel or Northern Ireland, where the state believed it was in mortal danger from external and internal subversion. Langbein's quantitative analysis of torture cases is even more surprising: during the supposedly enlightened reign of Queen Elizabeth there were 63 cases of torture, as compared to the three under that reputed tyrant Charles I.

In discussing torture Langbein takes a narrowly legal approach which enables him to explain the origins and decline of torture on the Continent far more convincingly than in England. To define torture in legal terms is perhaps too limited, as the coercion of prisoners can run the whole gamut from bullying to brainwashing.

Like all good monographs, *Torture and the Law of Proof* not only does what it sets out to do well but points to new directions. It is a concise scholarly work that compares two legal systems with graceful ease and has provocative implications extending far beyond the sleazy world of rack, strap-pardo, and thumb screw.

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GIULIANO GLIOZZI. *Adamo e il nuovo mondo: La nascita dell'antropologia come ideologia coloniale, dalle genealogie bibliche alle teorie razziali (1500-1700)*. (Centro di studi del pensiero filosofico del cinquecento e del seicento in relazione ai problemi della scienza. First Series, number 7.) Florence: La Nuova Italia Editrice. 1977. Pp. ix, 635. L. 10,000.

Giuliano Gliozzi's volume is probably the longest Italian contribution to the history of early American studies after Antonello Gerbi's *La disputa del Nuovo Mondo: Storia di una polemica, 1750-1900* (1955)—available in a revised and enlarged English version entitled *The Dispute of the New World: The History of a Polemic, 1750-1900* (1973)—and *La natura della Indie Nove, Da Cristoforo Colombo a Gonzalo Fernandez de Oviedo* (1975). Its specific aim is to offer a comprehensive and exhaustive picture of the complex and variable attitudes taken by Europeans toward the natives of America from the Renaissance to the middle of the eighteenth century. The book is divided into three sections, dealing respectively with the interpretations of the newly discovered continent founded on the Old Testament ("nuovo Mondo e Vecchio Testamento"), the disputes about the age of the New World ("Antichità e novità del Nuovo Mondo"), and the impact of the new scientific mentality on these problems ("Nuova scienza e Nuovo Mondo"). This general frame is articulated in various chapters and subchapters, conferring an organic arrangement to the immense but largely well-known body of material discussed by the author.

The guiding principles are laid down in the introduction, when Gliozzi mercilessly attacks the scholars who have previously treated the same topic, sparing only Sergio Landucci's *I filosofi e i selvaggi, 1580-1780* (1972) and Eugenio Garin's masterly essay "Alla scoperta del diverso: i selvaggi americani e i saggi cinesi" (now included in *Rinascite e rivoluzioni: Movimenti culturali dal XIV al XVIII secolo* [1975]). It is precisely this *pars destruens* that appears to be the *raison d'être* of the present book, based on a rigorously Marxist approach. Indeed, while Gerbi's *The Dispute of the New World* was explicitly a commentary on a particular aspect of Hegel's thought, this volume, in its conclusion, appears to be a kind of gigantic footnote to Marx's interpretation of the colonial system as set forth in *Das Kapital*. Unfortunately, the controversy about the origin of the American natives had theological, astrological, moral, and literary implications that work against Marx's rigidly economic pattern of historical development. The result of this contradiction is quite paradoxical: Gliozzi has written a brilliant synthesis of previous studies (from Gilbert Chinard's pioneering works to Don Cameron Allen's and Gerbi's basic contributions), a scholarly book which fascinates its readers like a novel but leaves them skeptical about the interpretation of the material he proposes.

On closer scrutiny, Gliozzi's claim to follow in Garin's footsteps is rather questionable, since it seems difficult to reconcile the Marxist philosophy of history with Garin's insistence on the relevance of the Hermetic tradition (cf. particularly *Lo zodi-*

*aco della vita: La polemica sull'astrologia dal Trecento al Cinquecento* [1976]). Indeed, Garin's habit of explaining historical events on the basis of *Renovatio* (a religious idea) and related astrological speculations is more in tune with the "cultural" approach rejected by the author than with the pan-economic explanation advocated by Marx. Should we conclude from this fact that good scholarship cannot identify itself with any given credo? I am afraid this is the most important truth we can learn from this ambitious book, in which the author has systematically omitted consideration of all those "cultural" circumstances that contradict his materialistic assumptions. In view of this, we should not be surprised by the fact that Gliozzi does not even mention Leonardo Olschki's excellent book, *Storia letteraria delle scoperte geografiche: Studi e ricerche* (1937), which showed that the European travelers were unable to see the reality of America, because they constantly tended to superimpose literary schemes on their direct experience (e.g., the nightingale that Columbus wrongly attributed to the New World after the pattern of the *locus amoenus*, a widespread *topos* in Western literature).

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H. JAMES JENSEN. *The Muses' Concord: Literature, Music, and the Visual Arts in the Baroque Age*. Bloomington: Indiana University Press. 1976. Pp. xv, 259. \$12.95.

H. James Jensen has written a small book of large scope. He writes, he says, to increase the reader's appreciation and pleasure in the several arts of the "baroque" through greater understanding of the historical ideas behind these works. He avoids the contentious problem of defining the "baroque" and settles instead upon a flexible period running from the sixteenth through the eighteenth centuries, choosing examples more or less at random to illustrate his arguments. Unfortunately, as he correctly says, "this book is not a tightly organized argument in support of a clearly defined thesis." It is rather a mixed assortment of quotations and allusions drawn from a wide variety of sources that must, I fear, leave the reader somewhat at a loss.

Perhaps the author lost his own way. He was inspired to his task, he says, by the desire to understand a Dryden poem by discovering the original ideas behind it. It seems that these ideas lay essentially in two areas, "faculty psychology and the rhetorical process." Apart from the fact that neither set of ideas is peculiar to the baroque and cannot serve therefore to define it except in the most general way, Jensen seems to have forgotten

that he meant to elucidate particular works of art and spends most of his time doing the reverse—trying to arrive at some general ideas in the period by assembling and classifying his examples. This leads him to his characteristic method of taking some general theme and then hopping about geographically and chronologically to enumerate instances, heedless for the most part of distinctions and differences. “Addison’s writings, Vivaldi’s music, Rubens’s paintings, Shaftesbury’s aesthetics, and Coiffetau’s psychology all help to elucidate 17th century ideas,” and Joshua Reynolds, even Marsilio Ficino, may help us to understand better a Restoration poem than an English contemporary. Unfortunately, Jensen forgets most of the time to show us how this may be done as he rushes through his plethora of illustrations.

Where there is a rare attempt at explication, as for example of Rubens’ *The Education of Marie de Medici*, it does not engender confidence. Jensen cites it as an example of the “rhetorical process” in painting and uses Felibien to examine it. We are told that “the small size of Marie de Medici herself draws attention to her” (p. 64), though even the reproduction shows that it is the lighting, perspective, and the arrangement and gestures of the other figures that does the trick. Even less convincing is his notion that a painting can be shown to exhibit a rhetoric similar to literature because it is “harmoniously” arranged. Jensen has trouble with a waterfall which he admits is not very harmonious but saves the day by finding its effect “more exalted than that of the regularized beauty of the three graces.”

Historians stumbling upon this book will find it a disappointing example of a genre that is unhappily growing more common. They should be warned that this is neither intellectual nor cultural history, even as our colleagues know it in literature or the arts. Fortunately, there are still the works of Basil Willey and Wylie Sypher or Louis Bredvold’s *Intellectual Milieu of John Dryden* to fall back upon, to mention only a few of the more worthy attempts to do something of what Jensen may have had in mind when he set out upon his task.

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WERNER KÆGI. *Jacob Burckhardt: Eine Biographie*. Volume 6, *Weltgeschichte, Mittelalter, Kunstgeschichte: Die letzten Jahre, 1886–1897*. Basel: Schwabe Verlag, 1977. Pp. xxi, 898. DM 96.

This volume should have concluded Werner Kaegi’s monumental biography of the great historian. However, the art historical materials in the Burckhardt archives proved to be more compre-

hensive and interesting than he had originally thought. He has put off until a seventh volume not only the index but also Burckhardt’s posthumous *Griechische Kulturgeschichte*. Volume six begins with another posthumous work, *Weltgeschichtliche Betrachtungen*, describing in 120 pages the genesis of Burckhardt’s reflections on world history. Kaegi shows how slowly and painfully Burckhardt’s ideas matured. *WB* began in 1868 as a chronologically structured introduction to history, focused on freedom and power, which emphasized change. It took on a systematic-morphological order, as Burckhardt progressively thought through the continuities of history: state, religion, and culture. Kaegi finds little in the archives to indicate that Hegel influenced Burckhardt. Moreover, Burckhardt’s plural system of distinct, autonomous historical forces is entirely different from the unified systems of Hegelian idealism. Kaegi also briefly surveys *WB*’s reception up to the present day. Almost from the beginning it has been misinterpreted, usually due to *parti pris*. Kaegi suspects that Benedetto Croce wrote his criticism of Burckhardt’s putative pessimism and *apolitie* without having read the book.

Kaegi’s second topic in this volume is Burckhardt’s lecture course, *Kultur des Mittelalters*, “one of his capital achievements,” offered at the University of Basel in 1882, 1884, and 1886. It focuses on the problem of cultural continuity from late classical antiquity to 1000 A.D. Christian orthodoxy, the papacy, and monasticism, Burckhardt insists, are the reasons why the Church was the only thing that held together as the empire fell to pieces. The unpublished manuscripts and voluminous notes for this course demonstrate Burckhardt’s close familiarity with medieval civilization. They convincingly refute Stadelmann’s thesis that, due to experience of the Renaissance in Italy, Burckhardt gave up the Middle Ages entirely.

Burckhardt composed a large new section on Islam for this course. His views were not improvised in the 1880s, however, but slowly matured through the years. Islam, he contends, cut off its adherents from the higher purposes of life. Its political expression is despotism, although sometimes mild and cultivated. Its essential religious spirit is also tyrannical but capable of assimilating genuine devotion, mysticism, and philosophy. Burckhardt discovers two “ideal types” in Islam with whom he has empathy: the chivalric and heroic prince, such as Saladin, and the ascetic dervish, who represents spiritual independence.

Burckhardt brings a more ample humanist perspective to bear on Byzantine history from Justinian to the Isaurian emperors. Among the first to break with Gibbon’s view of a millennial decline, he appreciates the empire’s defensive capacity, po-

litical unity, and stable monarchy. He also gives the Church credit for the empire's extraordinary ability to Hellenize its heterogeneous population, "a question ultimately not of blood but of spirit." Iconoclasm aroused in Burckhardt lively personal concern for "man's right to express his deepest insights and hopes in the visual medium." A pronounced iconodule, Burckhardt denounced Leo the Isaurian's "dangerous experiment" as esthetic as well as theological tyranny.

Impressive evidence of the vigor of the last third of Burckhardt's life is his career as professor of art history (1874-93), teaching Western art from its ancient Near Eastern roots to the middle of the eighteenth century. Art history was for Burckhardt "a free, personal creation, an intellectual whole" (p. 280), rather than a science. Abreast of contemporary scholarship, he studied the Pergamum Altar in Berlin two years after it had been discovered in Asia Minor. He shifts effortlessly from purely formal analysis of works of art to cultural history of their political, social, and religious setting. Turning from the psychology of the artist to that of the patron, he widened art's bridge to cultural history. In the development of themes, however, he emphasized that art history does not simply run parallel to cultural history but experiences its own high points and final periods. His lectures on fifteenth-century art cast doubts on the possibility of any generally valid concept of a European Renaissance. They also show that realism, individuality, and consciousness of the world and self, the marks of modern man, appeared in fifteenth-century Flemish art more strongly than in Italian.

The final chapter devoted to Burckhardt's last decade reveals a cheerful, inwardly harmonious, and intellectually active person consulted by colleagues, venerated by fellow citizens, and amused by children and his cat, Miggi. Forced by asthma and heart trouble to retire from teaching in 1893, he continued work on several art historical monographs, published posthumously. Kaegi's pages on these last years are among the finest he has written. Inspired by genuine *pietas*, they are informed by profound understanding of Burckhardt and his setting. Masterfully avoiding the pitfalls of sentimentality, on the one hand, and cool clinical indifference, on the other, Kaegi keeps Burckhardt at just the right critical distance for us to believe in the portrait he draws.

CHARLES H. O'BRIEN  
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ALAN S. MILWARD and S. B. SAUL. *The Development of the Economies of Continental Europe, 1850-1914*. Cambridge, Mass.: Harvard University Press. 1977. Pp. 555. \$22.50.

This book is the sequel to the same authors' *The Economic Development of Continental Europe, 1780-1870* (1973). As they explained in the preface to that volume, they initially intended to write a single-volume textbook on the modern economic history of Continental Europe before World War I, but as their notes accumulated they decided to go to the two-volume format. The earlier book even included a table of contents of the second volume, not strictly adhered to in the present publication. The authors do not explain the four-year delay in publication nor the change of title (nor the change of the American publisher). These seemingly trivial details might be overlooked except that they almost vitiate the principal—and laudable—purpose of the two books, that of serving as an advanced text on a subject that has been, until recently, poorly dealt with in English. First, however, one should be made aware of the genuine merit of the volume under review.

For the areas and topics treated (imperfectly indicated by the title) it is comprehensive, it is detailed, and it presents an able synthesis of much of the most recent literature. The authors, both British, are acknowledged experts in their fields, and together they command a wide range of languages as well as a reasonable familiarity with the necessary concepts of economics. One of the most notable features of the book is its attention to the smaller countries of Europe, the Low Countries in particular, and to the economically less developed nations such as Spain, Italy, the Austro-Hungarian Empire, and southeastern Europe. Although by no means in the mode of the so-called new economic history, the book contains a wealth, possibly an overabundance, of quantitative information. Factual errors are few and relatively unimportant; for example, the authors consistently confuse the Austrian Credit-Anstalt, founded in 1855, with the Boden-Credit-Anstalt, founded in 1863 (pp. 320, 499 *inter alia*); and "the main rivers of Siberia . . . flow north to south . . ." (p. 422). Specialists in particular areas and periods will no doubt find other such errors, and will dispute the authors' interpretations and analyses of specific episodes and processes. But that is all well, for the authors usually indicate rival interpretations and present sufficient information so that the alternatives can be weighed, at least provisionally. The more fundamental difficulty lies with the organization of the volume, and with its relation to its predecessor.

To serve as textbooks the two volumes will have to be used together. Even then there will be problems. The authors forsook the opportunity for a genuinely comparative economic history, opting instead for parallel histories. The predecessor volume contained chapters dealing with the institu-



tional framework, population growth, and technological change for Europe as a whole, before launching into parallel national economic histories, but the volume under review goes at once to the economic histories of individual countries or regions. Only the last two chapters, entitled respectively "International Trade and Investment" and "The Nature of Economic Development in Europe," deal with the continent as a whole. Moreover, there is a curious asymmetry in the presentation of national histories. The Scandinavian countries are scarcely mentioned in this volume since their history to 1914 was recounted in the predecessor. Russia, on the other hand, which was scarcely mentioned in the previous volume, rates two chapters—before and after 1860—in the present. The previous volume had a chapter on "Belgium and Switzerland in the Early Nineteenth Century"—no mention of the Netherlands—whereas the present has a chapter on "Belgium and the Netherlands," with no mention of Switzerland. Southern and Eastern Europe, almost wholly omitted from the previous volume, obtain very full treatment in the present one, at least from 1850 to 1914.

I, for one, would gladly have sacrificed some of the monographic details for a briefer, more synoptic survey. Nevertheless, one cannot deny that the two volumes together constitute an impressive synthesis that will be useful to professors if not so accessible to their students.

RONDO CAMERON  
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M. I. MIKHAILOV. *Bor'ba protiv bakunizma v I International* [The Struggle against Bakuninism in the First International]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 349. 1 r. 38 k.

The appeal of the Leninist variant of Marxism has always been in inverse proportion to that of anarchism and anarchists. The loss of face and faith experienced by Marxist-Leninists as a result of Khrushchev's secret speech in 1956 and the events in Eastern Europe has led to renewed interest in anarchist alternatives and their chief advocates. This in turn has forced Soviet scholars and propagandists to devote more attention to refuting the views of their rivals on the extreme left of the revolutionary spectrum.

M. I. Mikhailov, the author of a useful study of Marx's Communist League in the middle of the nineteenth century, has made a valiant effort to deal with one of the most important episodes in the dispute between Marxists and anarchists. He examines Bakunin's public life and involvement in the First International, and then deals with the

struggle against the Bakuninists in Italy, Spain, and Switzerland. An account of the jockeying for position on the eve of the Hague congress (1872) is followed by a chapter on the congress itself, and on the conflict between libertarian and state socialists in the final years of the International.

The picture that emerges will be familiar to readers of crude Marxist interpretations of the famous quarrel. The anarchists are condemned for ideological confusion, deluding the workers, and arresting the advance of Marxism. Mikhailov accuses them of adventurism, sectarianism, ultraleft phraseology, and contributing to political apathy among the masses. Hence Marx and Engels were fully within their rights in exposing Bakunin.

This thesis is supported by material largely drawn from Western Marxist and Soviet printed sources. The anarchist case and objectives are presented in a way that caricatures what Bakunin and his associates stood for and actually did. Few anarchist sources are used; there is no indication that Mikhailov has consulted the rich collection of documents which Max Nettlau deposited in the International Institute for Social History in Amsterdam. The hostility shown to Bakunin is extended to any scholars who do not accept the prevailing Soviet "line." Mikhailov's strongest criticism is reserved for Arthur Lehning, the foremost authority on Bakunin in the West. "Bourgeois historians" such as Paul Avrich and Samuel Baron are taken to task for suggesting that Lenin may have borrowed from Bakunin. For good measure Mao Tse-tung is attacked in the introduction and Marcuse in the conclusion.

Robust criticism and forthright condemnation of anarchism and anarchists throughout the monograph leads up to a generous tribute to Marx and Engels. Mikhailov's work ends with a reminder of the debt owed to them by "Marxist-Leninist Communist parties," which represent "the vanguard of progressive mankind, fighting for peace, democracy and socialism."

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GERHARD BOTZ et al. *Im Schatten der Arbeiterbewegung: Zur Geschichte des Anarchismus in Österreich und Deutschland*. Foreword by KARL R. STADLER. (Schriftenreihe des Ludwig Boltzmann Instituts für Geschichte der Arbeiterbewegung, number 6.) Vienna: Europa Verlag. 1977. Pp. 190.

Anarchism has always been an intriguing subject to historians and political theorists. The link between the Marxist labor movement and anarchism is doubly fascinating because of the contradiction: between socialist and Communist doctrine and the



"great leap" most anarchists want to take to a classless and decentralized society.

Anarchism in Germany and Austria between the late nineteenth century and the post-World War I period is a topic of which too little has been known until now. The three authors have therefore done an excellent service to historical scholarship by opening up an area on the edge of the Austro-German labor movement which requires close attention. While Gerfried Brandstetter has contributed a splendid chapter on the connection between Social Democracy and anarchism between 1889 and 1918, Gerhard Botz and Michael Pollak have concentrated on the Austrian-born anarchist Carl Dopf (1883-1968) to illustrate the shadowy and pseudoromantic existence of the anarchist movement on the very fringes of the labor movement. While Dopf cannot be compared with the great nineteenth-century anarchists, men like Bakunin, Tolstoy, and Kropotkin, his writings contain a mélange of ideas found in Sorel, Nietzsche, Tolstoy, Stirner, and others. The curious thing about such little-known thinkers as Dopf is the difficulty one has in deciding whether his cry in the wilderness is heard from the extreme left or the extreme right. Much of Dopf's thinking is reminiscent of Georges Sorel and his syndicalist ideas. The concept of social myth, which is apparent in Dopf, is traceable to earlier irrationalists like Schopenhauer, and emerges again in a grotesque fashion in National Socialism. The societal somersaults the world has made since 1914 have resulted in a diminution of confidence in rational institutions, and it is in these moments of history that the concept of social myth finds a hearing.

As far as Central European anarchism is concerned, it was so inextricably infiltrated by what today is known colloquially as "freak culture" that whatever valuable ideas for a reorganization (or de-organization) of humanity it may have had were obscured by irrelevant antiestablishment admixtures, including religious, sexual, dietary, and occultist hobby-horses of all kinds. Thus no one could clearly understand what lay behind the membership of these miniscule splinter groups in which anarchists and syndicalists were sitting at the same table with obscurantist religious revivalists, nudist culture proponents, free lovers, extreme vegetarians, and other "far-out" rebels. This is a pity because there are nuggets, gilded if not golden, worth noting in the journalistic work of men like Dopf.

Aside from the three brilliant essays, the book contains an introduction and a summary which lend cohesion and unity to the work. The sociological, ideological, and sociopsychological aspects of anarchism are analyzed in the summary and give the reader a necessary perspective on anarchism as

a stepsister of the labor movement, a sibling which has often been derided but can never be ignored completely.

ROBERT SCHWARZ  
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YISRAEL GUTMAN and LIVIA ROTHKIRCHEN, editors.  
*The Catastrophe of European Jewry: Antecedents—History—Reflections; Selected Papers*. New York: Ktav Publishing House. 1977. Pp. x, 757. \$17.50.

Most of the major studies of the "final solution," such as those by Gerald Reitlinger and Raul Hilberg, rely almost exclusively upon German documents and sources. As works in European or German history, they tend to reduce the Jew to a minor role in the Holocaust—a passive and faceless victim who is the object rather than the subject of history. Well-meaning attempts by survivors and amateur historians to correct this distortion by chronicling the Jewish experience during the Holocaust have succeeded only in creating what one Jewish historian has called "catastrophe" literature—lachrymose, highly emotional, and oblivious to historical methodology. In contrast to such works, the present volume of essays by responsible memoirists and Jewish historians, published under the auspices of the Yad Vashem Institute in Israel, the central repository of Holocaust documents, seeks to counterbalance the German bias of general studies by presenting academically sound examples of the "Judeocentric" approach to the study of the "final solution."

The work is divided into three sections. Section one, "Antecedents," includes essays on the rise and significance of modern anti-Semitism. Section two, "History," focuses on the period of the Holocaust itself and examines both the implementation of the "final solution" and the Jewish response. The final section, "Reflections," contains articles on research methodology and on the philosophical and religious ramifications of the Holocaust. The essays presented include selections from previously published works, colloquium papers, personal reminiscences, and scholarly articles, and reflect extensive use of Jewish as well as general sources.

As with most anthologies, the quality of the essays varies widely. A number of articles appear to be included for political or personal reasons rather than for their contribution to historical understanding. One wonders, for example, why it was necessary to include separate essays on both the ZOB (Jewish Fighting Organization) and Betar in the Warsaw Ghetto uprising. Other essays, preparatory studies for larger works, seem tentative and at times confusing in their conclusions. The contribution by Uriel Tal on Christian anti-

Semitism, for example, is often difficult to understand, and its major points were obviously reevaluated and reworked before they were incorporated into the author's excellent book on Christians and Jews in the Second Reich. Indeed, the inclusion of a number of essays that were later expanded into full-length monographs and the fact that more recent scholarly works on the Holocaust such as Lucy Dawidowicz's *The War against the Jews* make extensive use of Jewish sources might lead historians of the "final solution" to question the need for such an anthology.

But if the work is only of minimal value to specialists, it does serve a useful purpose in introducing the general reader to recent trends in Holocaust scholarship. The selections by Yitzchak Arad and Isaiah Trunk on the Jewish response to the "final solution" are particularly enlightening, avoiding the bitter polemics generally associated with the subject by carefully analyzing the complex internal and external factors affecting the nature and degree of Jewish resistance. Similarly, the testimony of Salo Baron at the Eichmann trial concerning European Jewry on the eve of the Holocaust (included in the section on "Antecedents") sheds important light on the prehistory of the Holocaust, a subject often ignored in general studies. The general reader will also learn much from the articles on historical methodology by Philip Friedman and Leni Yahil which, though emphasizing the need for a "Judeocentric" approach to the study of the "final solution," speak of the place of the Holocaust in both general and Jewish history.

An exhaustive and highly useful chronology which parallels general European developments and events pertaining to the Holocaust concludes the work. Given the nature of the audience, it is unfortunate that the editors did not preface the anthology with an explanation of the significance of the various articles and a brief description of their authors' academic qualifications. Nevertheless, the work is to be welcomed for the insight it offers the nonspecialist into the fascinating and complex nature of Holocaust research.

DAVID WEINBERG  
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JAMES THOMAS EMMERSON. *The Rhineland Crisis, 7 March 1936: A Study in Multilateral Diplomacy*. Ames: Iowa State University Press. 1977. Pp. 383. \$15.00.

Hitler described the period 1933 to 1937 as the "age of *faits accomplis*." The most startling and far-reaching of these was the German remilitarization of the Rhineland on March 7, 1936, by which Hitler destroyed whatever strategic advantage re-

mained to the Allies from their victory in 1918. Despite an almost complete awareness of Hitler's intentions, the Western democracies did little or nothing to prevent remilitarization of the Rhineland and precious little in the wake of the coup.

James Thomas Emmerson, professor of Journalism and Mass Communications at Iowa State University, offers the first detailed study in English of remilitarization. In nine well-organized chapters, the author takes his readers through the background of European security efforts in the twenties and thirties, Anglo-French anticipation of a Rhineland coup, Hitler's preparations, the coup itself, and, finally, the diplomatic backwash from the entire affair. Emmerson has made excellent use of published and unpublished sources available to him in London. His prose serves him well, as do his copious footnotes. (One might wish, however, that not just Emmerson but all scholars working with diplomatic documents would include more information in their citations than simply the date and frame/document number.)

Emmerson is at his best when he deals with Anglo-French diplomacy. His discussion of Anglo-French negotiations surrounding Hitler's coup is excellent. Britain was the key to the Western response to Hitler; France could not act alone, witness the cold douche of Gamelin's pathetic suggestion for the symbolic occupation by France of a one- to eight-mile-wide sliver of German territory between Saarbrücken and Merzig. The French military planners seemed to be testing the strategy of *drôle de guerre*, employed during the winter of 1939-40. Naked British weakness disabused Flaudin of his delusion that together Britain and France might respond forcefully to Germany's violation of Locarno. Both Paris and London were deeply impressed with what they took to be the already well-advanced state of German rearmament, a misapprehension conveyed to the French and British people and reflected back in Anglo-French public opinion. In the wake of the coup, the only constructive element in Anglo-French conversations was the British note of March 19, laying the foundation "for eventual creation of a defensive bloc which would have committed London to an anti-German alliance" (p. 243).

The confidence with which Emmerson approaches British and French affairs is not evident in his treatment of Germany, either in his use of sources or in his language. Too often, his language is speculative: Hitler and the Germans "may have," "probably," "appeared to." Emmerson's admirable use of primary sources is compromised by his heavy reliance on memoirs and secondary works more than ten years old. Surprisingly, Emmerson chooses to cite only two books and one

article published since 1967. The past seventeen years of scholarship on Hitler are ignored, as are such works as Weinberg's indispensable treatment of Hitler's foreign policy from 1933 to 1937, Jacobson's monumental study of National Socialist foreign policy, and Hildebrand's provocative essay on Hitler's foreign policy. Of particular help would have been the early results of work currently in progress in Germany concerning Hitler's foreign policy goal-formation, available in: Manfred Funke, editor, *Hitler, Deutschland und die Mächte* (1976). Thus the overall effect is of a book which is already ten years old.

Emmerson concludes that the remilitarization of the Rhineland became more significant as it receded into the past. As people began to perceive more acutely the threat of Hitler, the Rhineland episode took on new meaning not immediately apparent in 1936. "The general outlines of events [of the remilitarization of the Rhineland] are, of course, already familiar to historians of the period . . .," acknowledges D. C. Watt, who wrote the introduction to Emmerson's book (p. 14). Emmerson neither adds many new details to an already well-known episode, nor attempts to place the Rhineland coup within the larger context of Hitler's overall foreign policy. He does, however, provide us with the best recent treatment of the Anglo-French diplomacy surrounding the crisis.

MARSHALL M. LEE  
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PATRICK FRIDENSON and JEAN LECUIR. *La France et la Grande-Bretagne face aux problèmes aériens (1935-mai 1940)*. Vincennes: Service Historique de l'Armée de l'Air. 1976. Pp. iii, 208.

This monograph makes a small but useful contribution to the steadily growing body of studies concerned with the relationship between strategic planning and appeasement in the 1930s. Based on an examination of the French air, naval, and army records, the archives of the French Foreign Ministry, Chamber of Deputies, and Senate, the Daladier papers, and Air Ministry archives in the Public Record Office in London, it describes and analyzes Anglo-French relations in the years between the Abyssinian crisis and the eve of the collapse of France through a close scrutiny of the discussions on problems of air defense. It thereby supplements and benefits from the work of the several colloquia on Anglo-French relations organized since 1971 by the British and French Committees on the History of the Second World War, and in the spirit of such endeavours it has profited from the not inconsiderable work done in this field by non-French scholars.

Military conversations between French and British air staffs began in 1935 and over the next four years were more frequent than either naval or army discussions. In large part the initiative and impetus came from the British, who needed at least a minimum of French cooperation for bases in France from which their bomber force could effectively attack the heavy industry of the Ruhr. But progress in such talks and in joint planning was severely circumscribed by the failure to develop a joint strategy for the employment of air power. This failure in turn reflected both the political differences between the two powers on the question of relations with Germany and Italy and their common conservatism, which meant that neither was prepared, although planning for a long war, to mobilize its economy sufficiently to produce the required number and type of aircraft. Interservice rivalries on both sides further contributed to the failure to evolve a common strategic doctrine in the air, and each country put the problem of its own defense first.

If the main impetus for talks came from the British, was it then a case of the British serving only their own interests? Is it true, as voices in the French High Command were to allege almost as soon as the armistice was signed, that the RAF reneged on its prewar commitments during the Battle for France in May and June 1940, and thereby bore responsibility for the defeat? In addressing themselves to this specific and much debated question, the authors demonstrate conclusively that such accusations were groundless and represented a pathetic attempt by the High Command to shuffle responsibility for defeat onto British shoulders. On the contrary, the authors aver, so far as fighters were concerned the British went beyond the commitments they had made in prewar discussions. In any case the French had been informed of and fully appreciated British concern about a German bomber offensive, and because no one could predict where it would strike first, no commitment could be or was made beforehand to tie down a British fighter force in France. The British refusal to accede to the most extreme demands of the French High Command in June 1940 was, therefore, perfectly consistent with their prior commitments, utterly predictable, and, in the authors' view, immaterial to the ultimate outcome of events. The myth of *la perfide Albion*, at least where the RAF is concerned, should be laid to rest by this study.

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LILLY MARCOU. *Le Kominform: Le communisme de guerre froide*. Paris: Presses de la Fondation nationale des sciences politiques. 1977. Pp. 343. 120 fr.

The Communist Information Bureau (Cominform) was established in September 1947 and was dissolved in April 1956. It published a journal entitled, *For a Lasting Peace, For a People's Democracy*, served as Stalin's vehicle for excommunicating Tito in 1948, but never acquired a bureaucracy as had its predecessor, the Comintern. This book analyzes its history, finding that it was much more than just an instrument of Soviet control. Lilly Marcou claims that the establishment of the bureau was a "normal" expression of the mentality of the European Communists, who felt themselves increasingly isolated between East and West and craved the reassurance of unity. She points out that the ritualism of the journal, the cult of the Soviet Union and of Stalin's personality, and the expressions of hatred for outsiders and "saboteurs" were functions not just of orders from above, but of a traumatized siege mentality below which was an integral part of the twentieth-century European political experience. In the end, with the totalitarian model firmly out of the way, Marcou goes back and studies what the Balkan Federation project of 1947, the Yugoslav-Soviet conflict, the Greek affair, the great trials of the early 1950s, and the peace movement suggest about Stalin's foreign policy, which she finds on the whole conservative and anti-insurrectionist.

The author depends on the French, Romanian, Italian, and English languages, using few Slavic and German sources, yet she has taken the trouble to probe into the contemporary Communist press and to talk to party historians and former Communist politicians in Paris, Budapest, and Bucharest. As a result, she has uncovered all sorts of fresh material and demonstrates, above all, how much can be done in the history of Stalinism by an intelligent and persistent researcher. But the book also suggests how much remains to be done. "Everything leads one to believe," writes Marcou, "that [Stalin] just after the war favored the idea of 'popular democracy' . . . intermediary between capitalism and socialism" (p. 37). The "everything" is a long series of remarks by Stalin in public and in private to visiting Communists. But the author cites only Isaac Deutscher's revised biography of Stalin which is superficial on the postwar period, because she does not have time to write the Soviet part of the story herself, and because there exists no comprehensive study of Soviet policies in the 1940s. The huge void in the literature disorients her far less than it has the American historians of the start of the Cold War. But her example demands that someone go back to read the Soviet press for the postwar years with the same eye she has cast on *Scinteia* and *L'Humanité*.

Marcou's discussion of the trial of László Rajk in Hungary in 1949 brings to light another major

unexplored problem. She takes into account the obvious evidence that the charges brought against Rajk were fabricated. She cites the formerly prominent Hungarian Communists who swear that during the postwar period there was no Rajkist political opposition to Rákosi, the head of the Hungarian Communist Party, only "personality conflict." She cites also Rajk's own synoptic testimony to a codefendant in prison just before his execution. All this suggests the trial was a Soviet machination, nothing more.

Yet in Budapest in 1973, a member of the Party Politburo told Marcou how Rákosi telephoned Stalin on May 30, 1949 announcing: "I've just arrested Rajk. What shall I do?" and received the reply: "That's not for me to say. It's up to you Hungarian Communists to decide. You're the ones who should know what to do" (p. 271). Further, a recent biography of Rajk published in Budapest (which Marcou does not cite) tells that in Hungarian Politburo circles Mihály Farkas and Gábor Péter, Rákosi's closest collaborators in the party and the police, were talking about Rajk as "the enemy" as early as 1947 and presented formal charges against him early in 1948, a full eighteen months before his arrest. And there is a mass of evidence in the Hungarian Communist press of 1947, the reportage of contemporary observers, and in the writings of Marcou's historian friends in Budapest that Rajk differed with Péter, Farkas, and Rákosi over the pace of the seizure of power, the party-police relationship, the methods of political education, and the shape to be given the national front. Clearly the trial was not just the result of Soviet machinations and "personality conflict."

Marcou intelligently escapes these contradictions in the evidence by recognizing the existence among the Communists of the Stalinist period of a war psychosis and traumatization which fundamentally distorted their perception of their own political actions, denying them an understanding of any middle ground between complete unity and dastardly treachery. If she is right, the entire history of Stalinism must be rewritten in terms of political conflicts within the movement which were real and visible, but which were somehow not perceived by the participants.

Marcou raises a third, and for historians the most important unexplored problem of the era when she finds that the notoriously dull articles in *For a Lasting Peace*, written in Politburo language ("politbiurski" the Russians call it), were read not by the masses, but by the inner-party elites themselves and constituted a primary medium through which the leaders of Communism in various countries communicated their inner feelings to each other in the ghastly isolation of the Cold War terror. By implication the public documents of



Communism—the daily party newspapers such as *Pravda* and the theoretical journals—are sensitive organs for inner-party communication and reliable sources for historians who wish to penetrate the inner secrets of the movement. In explaining the significance of “*politbiurski*,” Marcou does not go into the question of “ideological thinking”—of whether the existence of doctrinal agreement among Communists makes possible the elliptic communication in the press. Nor does she go into the administrative function of the public ideological discussions—indeed she tends to reject as too Machiavellian the probability that Stalin manipulated his public line as an important means of communicating with and controlling the actions of his followers. But she effectively shows that significant tools for understanding the politics of Stalinism are in our hands, and have been for many years, waiting to be used.

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LOUIS A. KNAFLA. *Law and Politics in Jacobean England: The Tracts of Lord Chancellor Ellesmere*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1977. Pp. xxvii, 355. \$34.00.

This study of Thomas Egerton, Lord Ellesmere, Lord Chancellor 1603–17, is an notable contribution to our growing awareness of the importance of secondary public figures in Jacobean England. Louis A. Knafla begins with a sketch of Ellesmere's background in the north Welsh borders and his career until the death of Elizabeth. Ellesmere's studies at Oxford and Lincoln's Inn are considered in detail as the basis of his approach to the law and its problems. Ellesmere not only delved into the evolution of law in England, but also studied the works of Continental students of law. Becoming Master of the Rolls in 1594 and serving as Lord Keeper 1596–1603, Ellesmere bent his considerable administrative talents and energy to improving the operation of Chancery, both as a court and as a department.

The emphasis in this work is on Ellesmere's career as Lord Chancellor under James I. Part two contains carefully edited texts of eight of Ellesmere's tracts, all dating from the Jacobean period. Knafla's analysis of Ellesmere as a Jacobean public official in part one is based on these tracts. A central question with which the Lord Chancellor wrestled concerned sovereignty and the prerogative. Contrary to the generally-accepted doctrine of the king's two bodies, natural and politic, Ellesmere postulated one single unified “natural body,” expressing itself partly in his private will

and judgment and partly in the law, both customary and statutory. This concept, expressed here most fully in Ellesmere's tract on the *postnati*, does not emerge clearly, perhaps because Ellesmere himself did not bring it into sharp focus. In Parliament, particularly in 1610 and 1614, Ellesmere was a moderate, trying to promote cooperation between king, Lords, and Commons by emphasizing common concerns, tact, and timely concessions. After the failure of 1614, Ellesmere advocated administrative and financial reforms in surprisingly close cooperation with Sir Edward Coke. Although Ellesmere seemed to make little progress, his proposals bore fruit in the fall of Somerset and the Howards, the reform of the navy, and the revival of Parliament in 1621.

Ellesmere also tried to reform the law. The court system faced the common law's increasing reliance on legal fictions to meet changing needs. Ellesmere believed that legal fictions complicated procedure and introduced uncertainties into the common law. Coke, on the contrary, relied on legal fictions to adapt the law to change without the appearance of altering it. In the clash between the common law courts and the prerogative courts, Ellesmere defended local and regional jurisdictions against attempts to draw cases into the central courts of common law. Ellesmere also vigorously defended the ecclesiastical courts against the common law courts. The final clash with Coke, who attempted to subordinate the Court of Chancery to the courts of common law, was in part an ironic result of Ellesmere's success in reforming the Court of Chancery. By 1603 this court was cheap, speedy, effective, and popular. The dispute came to a head in the last years of Ellesmere's life. By a penetrating analysis of the relevant statutes, reflecting his early university and legal training, Ellesmere demolished Coke's position and won a complete victory.

In this study Ellesmere emerges as an energetic and well-intentioned official. His plans for reform were frequently thoughtful and sensible. When he was in a position of authority, as he was in Chancery, he eliminated corruption and promoted efficiency. But outside his own department he accomplished little except to bridle Coke, and even this triumph was a Pyrrhic victory. Although a fuller analysis of the reasons for Ellesmere's limited success would have been welcome, Knafla has added a valuable dimension to James' reign.

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HENRY HORWITZ. *Parliament, Policy and Politics in the Reign of William III*. Newark: University of Delaware Press. 1977. Pp. xiii, 385. \$22.50.



Henry Horwitz has set out to provide us with a narrative political history of William III's reign. In doing so he has sought to bridge a gap in the literature left unfilled since Macaulay's abortive effort. Several studies, Feiling on the Tory party, Kenyon on Sunderland, Baxter on William III, Ogg on the whole reign, have provided valuable new insights. Horwitz now enables us to learn, day by day, how political affairs were transacted, how political bargains were consummated. The focus is on "how the 'King's business' was transacted in parliament and on tracing the origins and fate of those bills whose authors aimed to reform the 'constitution', regulate the conduct of politics, and improve the workings of the royal administration" (p. viii). Diplomacy, wartime strategy, and non-political aspects of the reign are only mentioned where they impinge upon politics at Westminster. Above all the work is an unrivaled source for the business of Parliament. Horwitz has been assiduous in gleaning scraps of information on debates and negotiations that permit him to present a detailed, coherent, and convincing account. The careful detail does not always make for light reading, but rather for a compendium of facts, a reference work for students.

In summary paragraphs and chapters Horwitz analyzes the events he has recounted and comments on their significance for the evolving system of parliamentary government in England. He informs us that "King William's was the single most important influence on the politics of the day" (p. viii). In describing the impact of the war on the changing constitution, William's efforts as first minister (poor), the attempts of the Commons to control the executive, the rise and critical importance of Robert Harley, and new evidence for the inception and role of the cabinet, Horwitz has made important new contributions to our knowledge of the period.

Because of the crowded canvas, the mention of virtually every political figure of the reign (whether of major or minor importance), there are few revelations about the character or motivations of individuals. It is also not a party history. He does emphasize the complex and transitional nature of party groupings, but he leaves it for the reader to follow the strands and threads in the weave of his narrative. He presents here essentially the same thesis he has set forth in earlier studies. One does find many illuminating comments, especially on the emerging Whig junto and its relations to its patrons. The strength of the book lies in its explication of the workings of the political system at the center and the vital changes that took place during the reign in the constitution. Above all Horwitz emphasizes the increasing authority of the Commons, its new sophistication in its dealing

with the executive, and how "most of the major constitutional proposals in the preliminary version of the Declaration of Rights had either been embodied in statute or accepted in practice" by the end of the reign (p. 314). This is an essential study for any student of the period.

HENRY L. SNYDER  
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J. P. KENYON. *Revolution Principles: The Politics of Party, 1689-1720*. (Cambridge Studies in the History and Theory of Politics; the Ford Lectures, 1975-76.) New York: Cambridge University Press, 1977. Pp. viii, 248. \$19.95.

It was once quite easy to master the motivations behind the Glorious Revolution of 1688. One had merely to read Trevelyan and a few passages from Locke. Just as Robert Walcott, J. H. Plumb, Henry Horwitz, and the team of Speck and Holmes in their political studies have obviated Trevelyan, so now has J. P. Kenyon, building on the early findings of Charles Mullet, George Cherry, John Pocock, and Quentin Skinner and some new work by Mark Goldie, thoroughly removed Locke as the apologist for 1688. It was not that Locke spoke too democratic a language—that was something Locke never intended anyway, according to C. B. Macpherson—but that no one really understood what he was getting at. Besides, a focus on Locke has always led historians away from the volumes of pamphlets dating from these years; Kenyon has at last heroically used these as the proper sources for study.

In ten parallel chapters, Kenyon delineates the competing Whig and Tory philosophies on the Revolution, providing subtle insights into how the ideas fit into each decade's action. Much of his work is necessarily expository, yet one never has the sensation of merely following a digest of contemporary opinion. A surprise for this reader, one who is quite familiar with it as a sub-theme, is the dominance of religious considerations in Revolution theory. The events of 1688 were as much a dislocation in the heavens as in the court. It turns out that divine right, in its broadest as well as its most specific applications, continued to reign over political considerations, especially if a particular issue threatened Anglicanism. Geoffrey Holmes, in his study of Sacheverell, has recently displayed the potency of clerical political involvement, even more than had George Every, but few could have guessed the utter pervasiveness of religious—even theological—political ideas after 1688. Filmer was very much alive in Anne's reign, and so was Charles I, who is revived in a startling chapter 5, "King Charles's Head." Kenyon discovers that

"the more we study the controversial literature of this period, the more central does the execution of Charles I appear" (p. 66). In the back of all minds was James II's flight and the trembling prospect of what might have developed had William restrained the prisoner of Rochester, or had James won at the Boyne. Kenyon keeps his Tory ideologists in hot pursuit of their Whig counterparts over the issue of implied regicide, the hollowness of the Whigs' subterfuge of James II's abdication, the suppositiousness of popular right, and, of course, the eternal "Church in Danger" issue, to which Kenyon ascribes considerable influence. By the end of Anne's reign, the Whigs had to dump first their left wing, the populist survivors of republicanism, and then the Dissenters, over the issue of occasional conformity, to make an accommodation with Nottingham, and, finally, even after the triumph of 1714, to scrap any claim to popular leadership with their rascally Septennial Act. Walpole becomes merely the personification of the Whigs' betrayal of their already compromised "Revolution Principles."

Considering his seeming vindication of the solidity of Tory ideology, Kenyon's portrayal of Whig theory in his two most successful chapters, 4 and 7, is quite fair. There is a superb discussion of the Whig attitude toward "the People," and just what they could have meant by the term. There is eminent treatment of James Tyrrell, John Toland, and Algernon Sidney's influence and the real intention behind Walter Moyle's antiquarianism. But it is nothing but disintegration, according to Kenyon, for Whig theory, as the party's leaders forsake whatever principles they began with and become increasingly involved with the machinery of power from the Junto through the Hanoverian triumph.

There remain only a few observations on what is otherwise an impressive thesis. Kenyon's work could have displayed the points, and they were many, at which Tory and Whig doctrines coalesced, and he is forced to a much more partisan, subsequently gloomier, conclusion than necessary. For instance, the Anglican view was neither necessarily nor exclusively Tory. De factoism, conquest, and providential right could be found in many Whig quarters, and parts of these ideas were certainly behind the concept of James' abdication. Much of the Anglican position, as Margaret Jacob has so dramatically demonstrated, was part of a new wave of "Newtonian" thinking, which transcended the old Whig-Tory dichotomy of 1687-88—as opposed to Kenyon's interpretation of its conservative character. Yet Kenyon does not mention Jacob's discoveries. Kenyon refers to the Anglican position as Hobbist in the sense that it recognized any "settled" government as requiring obedience. But this is to miss the point of Erastian-

ism, which essentially regarded any act of state as legitimate, even Henry VIII's blatantly personal reasons for breaking with Rome. Caesar, even a pirate Caesar, is a law unto himself in things that concern only this world and its machinations. Though the state may at times be immoral, or amoral, the Christian's obedience to it is always a moral act.

Perhaps Kenyon scores the ultimate point about the Whigs' theory, however, when he compares their use of popular right as a justification for the Revolution to a deist's mode of reference to the clock-maker God: once having discovered that the people are the source of government, why bother having recourse to them again? The English constitution is self-regulating and self-balancing, just as the universe is; therefore, a knowledge of its origin in the people is just as useless as a deist's knowledge of God is to him in the study of the universe. This analogy is credited to John Brewer by Kenyon. Finally, Kenyon makes much of the possibility that the Whigs after 1714 deliberately abandoned the right of revolution when they realized it could become a Jacobite battle cry.

Kenyon's exposition will provide fuel for a score more of political studies, and his conclusions should keep us all busy for the rest of this century, or at least until 1988.

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L. T. C. ROLT and J. S. ALLEN. *The Steam Engine of Thomas Newcomen*. New York: Science History Publications. 1977. Pp. 160. \$15.00 or \$12.95 prepaid.

This book serves more than one purpose. In the first place it acts as a memorial by the Newcomen Society, the English body which began publishing papers in the early interwar years on the history of technology, to L. T. C. Rolt, their late vice-president. Rolt was a fine scholar, but not a professional academic, who wrote outstanding biographies of Brunel, Telford, and the Stephensons, as well as much on tools, engineering, and canals, and was greatly active in industrial archeology and in steam railway preservation. In 1963, for the three-hundredth anniversary of the birth of Thomas Newcomen, the most justifiable inventor of the steam engine, the society which had so appropriately taken his name asked Rolt to write a celebratory volume. Though perhaps not his best book, this summed up well the available factual information on the Newcomen engine, much of it coming from the Society's *Transactions*.

The idea of issuing the book again as a memorial to Rolt was a good one. But it is quite right that we should now have a second author and a changed

title. For it was in the most interesting part of the subject, the circumstances surrounding the life and inventive triumph of Newcomen and the early diffusion of his invention, that much research was under way as Rolt was writing. John Allen, the new co-author, and, like Rolt, an engineer-historian outside the academic world, was one of those making a particularly important contribution. In some ways Rolt was not entirely clear on the implications of his latest information, which continued research has since confirmed.

The revised book is thus a great advance on Rolt's, which is as he would have wished. Main areas of new research are in the details of Newcomen's personal connections and the building of the early engines, the membership of the joint-stock company which took over the rights in Savery's engine, and its origins in the Apothecaries' Company. Perhaps the most astonishing information is on the diffusion of the engine, around one hundred being built in Britain before the end of the patent in 1733, and a dozen in Europe. Over the eighteenth century as a whole nearly 1,500 Newcomen engines have been listed as against about 580 Watt engines, and the Newcomen list may yet become longer. Again, the engine aroused a vast scientific interest, and this can be shown by its description in many European scientific and encyclopedic works before 1760 and its frequent depiction in their plates. One of the great strengths of the revised book is its quite remarkable illustrations, including for instance not only these European items but early photographs of engines which survived into the era of the camera.

Occasionally the weld between Rolt's original and the new material is not perfect, and less pious hands might have been tougher in revising some of his views, especially on the engine's relation to contemporary science and scientists. However, Allen deserves the greatest credit for what is not a reissue but, in its central sections, a transformed book. It is a sobering thought that much of the remarkably researched new evidence comes from him and others who are not professional historians. At last Newcomen moves from under the shadow of Watt to be clearly exhibited as the deviser of an invention of vast economic as well as technological importance, the begetter of what Ferguson has rightly called a "phenomenally successful" machine.

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BILL WILLIAMS. *The Making of Manchester Jewry, 1740-1875*. New York: Holmes and Meier. 1976. Pp. x, 454. \$20.00.

The traditional cut-off point in considering the history of Anglo-Jewry is 1881, the year of the dreaded May Decrees in Russia which sent an endless stream of migrants to England and America in search of a haven from religious persecution. By terminating his study of the Manchester Jewish community in 1875 Bill Williams challenges the conventional notion that 1881 marks the beginning of a new era in which a significantly enlarged immigrant Jewish working class created tensions, both within the Jewish community and in the host community at large. Williams argues persuasively that in Manchester, at least, the mass migration and its attendant social problems were the culmination of a process that can be traced back to the mid-1840s.

Panoramic in scope, Williams' book is nevertheless rich in detail, offering fascinating insights into the internal development of the Manchester Jewish community and into the changing role of that community within a changing Manchester. On one level an illuminating account of the making of a great Victorian city, on another level *The Making of Manchester Jewry 1740-1875* is the history of a labyrinthine network of relationships and marriages among the Jews of Manchester. Personal relationships frequently reflected economic and commercial ones, and often Jewish involvement in retail trades and manufacturing placed Jews within the mainstream of Manchester concerns. Members of the Jewish community were active in the anti-Corn Law campaign, and their names can be found on the list of contributors to a fund honoring Cobden.

Williams traces the history of the Manchester Jewish community from the late eighteenth century when it consisted of no more than a few disparate Jewish traders, to the 1870s when Manchester had become the only provincial center to boast of synagogues serving Reform, Sephardi, and Ashkenazi congregations—the full range of Jewish religious persuasions. The development of the religious structure in Manchester offers a fascinating parallel with the economic development of the community. As its members achieved economic security and success they pressed for positions within the community which paralleled their financial positions. The proliferation of synagogues reflected not only diverse religious views, but the desire for self-determination, position, and prestige on the part of the congregants. This internal struggle for power accurately reflected the continual movement toward democratization which England was experiencing.

Williams' study provides a treasure-trove of information, insights, sound historical judgments, and useful statistics. Valuable as a chapter in the history of Manchester, it is equally valuable as a

study of an urban minority. The comparisons between London and Manchester Jewry and between Anglo-Jewry and the native community will be useful to students of all aspects of English history. This is urban history at its best.

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DAVID C. ITZKOWITZ. *Peculiar Privilege: A Social History of English Foxhunting, 1753-1885*. Brighton, England: Harvester Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. 248. \$19.50.

David C. Itzkowitz has written a lively, readable, judicious, and sympathetic account of the rise and decline of English foxhunting from its inception in its modern form to the agricultural depression of the 1870s. The author's purpose is twofold: to delineate the changing shape of the foxhunter's world and, more ambitiously, to cast light on social relationships in the nineteenth-century countryside.

In the pre-railway age, Itzkowitz argues, foxhunting was predominantly a community sport in which local landowners, gentry, farmers, and the occasional farmhand crossed ditches and jumped fences together. The myth of foxhunting as a sport for all and the view that hunting helped unite classes were therefore not totally without substance. Only the aristocratic young bloods of such prestigious Midland hunts as the Quorn and the occasional metropolitan hunter like the famous "Jorrocks" belied this notion of a local, community-based recreation.

Yet, with the advent of the railway, a sport that had already been made fashionable by Hugo Meynell and sporting journalism grew by leaps and bounds. Trains put many hunts within easy reach of the opulent urban dweller: the number of hunts increased, land was more intensively hunted, and solid burghers and women were found in the field. By the 1860s foxhunting was the fashionable recreation. As Itzkowitz deftly explains, the stresses generated by this change were considerable; larger hunts did more damage to farmland, and the floating population of hunters often failed to pay their way, thereby eroding the tradition of community sport.

Though foxhunting changed—it became more institutionalized, more expensive, and a more burdensome undertaking for the Master of Foxhounds—the myth, as Itzkowitz is at pains to emphasize, continued relatively unchanged. Though there were occasional criticisms of foxhunting and

the odd spectacular scandal, it was not until the agricultural depression of the 1870s that foxhunting came to divide rather than unite the rural community. Only financial hardship, it is argued, induced farmers to seek compensation for damages, insist on the use of wire fencing, and demand a greater say in the regulation of the sport.

Itzkowitz uses his account of foxhunting to emphasize the continuance and strength of such values as deference, paternalism, largesse, and a sense of community in rural society. Doubtless this is a useful corrective to the view that the countryside was riven with conflict, but at times the author protests too much. Its potent mythology was a necessary part of foxhunting precisely because the sport was a source of friction in rural England. Disputes and tensions could only be avoided by an extraordinarily obliging and congenial Master of Foxhounds, which was why his task was both so important and so difficult. Equally, the toleration by farmers of the sport is not necessarily a demonstration of the endurance of traditional values in rural society at large. The backslapping bonhomie of the hunt should not conceal the last agricultural laborers' revolt or the bitter fight over the unionization of the rural work force. Excellent as Itzkowitz's study is, one wonders if he would have drawn the same general conclusions if he had taken tithes or poaching instead of foxhunting as his entree into the bucolic world.

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ISAAC KRAMNICK. *The Rage of Edmund Burke: Portrait of an Ambivalent Conservative*. New York: Basic Books. 1977. Pp. xiii, 225. \$12.95.

When a book contains sloppy scholarship, innuendoes without evidence, forced interpretations, and opinionated generalizations, it almost has to be a "reinterpretation," as the jacket says, but for the same reason not a "major" or a credible one.

The title comes from a dubious premise: "at the heart of conservatism is rage" (p. 3); and Burke's often passionate language is offered as exemplification and proof. Whether the particular is derived from the general or the other way around is not clear. Burke's dislike of "theorizing about government," Isaac Kramnick generalizes, is "the eternal longing of the conservative for the elimination of rational thought from politics" (p. 23). What frothing cretins are Kramnick's bogeymen! Burke's ambivalence, according to Kramnick, was a love-hate relationship between the feminine, passive aristocratic principle and the masculine, aggressive bourgeois principle that complicated



Burke's public career. I do not find persuasive the justifications for the sexual equations. And despite impatience with some peers in his later life and bitter attacks on two of them, Burke did not give up the aristocratic principle anymore than, in his contempt for Henry VIII, he abandoned the monarchical principle. Burke, we read, was one of the first to see that "the social characteristics of bourgeois society matched the sexual patterns of assertive masculinity" (p. 141). He also recognized "the linkage between sexuality and capitalism, and their common modalities of aggression and exploitation" (p. 141). So sex raised identity problems, public and private. Kramnick discerns an oedipal complex existing simultaneously with his youthful friendships, suspect, with Dick Shackleton and Will Burke. The sojourn of the fragile child Burke in the country with maternal kinfolk is for Kramnick, by some strained deduction, desertion (p. 36) and abandonment (p. 63) by an irascible father. Tension between father and son provoked William Dennis to write of Edmund, "he forms desperate resolutions" (quoted p. 53). By carelessly omitting the word "often" before "forms" (Thomas W. Copeland, ed., *The Correspondence of Edmund Burke*, 1: 66, n. 2), Kramnick makes the resolutions seem even more desperate, and the change of meaning is congenial to his argument.

Sometimes evidence not in this book refutes its interpretations. Burke's "constant quest for an older male figure who would provide both affection and authority" was "a central dimension" in his "political attachments" to William Gerard Hamilton and Lord Rockingham (p. 99). That they could dispose of jobs is apparently irrelevant, and when Hamilton was Burke's age and Rockingham a year younger, the psychological profundity about older male figures becomes nonsense. For the "sexual referents" (p. 103) in the connection with Hamilton, no evidence is offered.

The case of Warren Hastings is a larger matter. In part to discourage people from thinking him like Hastings, Burke attacked this personification of "irresponsible, aggressive, and conquering masculinity" and of "the consequences of unleashed and unrepressed sexuality" (p. 134). By dubbing him the "bullock contractor" Burke supposedly linked "the upstart Hastings to the sexual Hastings" (p. 134). But corrupt contracts for bullocks were in the charges against Hastings, and more unhappily for the sexual interpretation, bullocks are not bulls. The sexual linkage extended to the Nabob of Arcot whose debt "was an awesome phallus" (p. 135). Burke acknowledged that it swelled, and that becomes sexism. Burke's passion also swelled, and was expressed in extravagant,

even scatological, language which Kramnick interprets in sexual terms, oblivious to the purposes of Burke the rhetorician.

On page 87 Kramnick says, "There is no solid evidence that can be produced here which would positively sustain the interpretation of Burke's sexual and psychic life offered in this book." That being so, it is asking much of a reader who values evidence to accept the interpretation without serious reservations.

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CHARLES RONALD MIDDLETON. *The Administration of British Foreign Policy, 1782-1846*. Durham, N.C.: Duke University Press. 1977. Pp. ix, 364. \$15.75.

Charles Ronald Middleton contributes this study to the not overworked area of administrative history. His purpose is to examine how foreign policy was administered—rather than its origins or nature—during the period between 1782, when the foreign secretaryship was created, and the year of the repeal of the Corn Laws. His story unfolds in a logical sequence and scrutinizes, in turn, the functions of all officials concerned with foreign affairs.

The foreign secretary himself began as a cabinet officer whose activities were reviewed by the prime minister and his colleagues in other departments. He continued in this ambiguous position until 1812, when Castlereagh achieved a considerable degree of independence in both initiating and carrying out foreign policy. While the position of the foreign secretary was being strengthened, that of the monarch weakened dramatically between the reign of George III, who exercised a strong voice in all important policy decisions, and Victoria, who could only consult, encourage, and warn.

There were usually two undersecretaries of state. They had no right to be consulted or duty to give advice, so their role in the creation of foreign policy was negligible. But during the post-1821 period, when the work of the Foreign Office expanded enormously, the permanent undersecretary emerged as an important administrative official who gave continuity amid changing administrations. On the lower levels toiled the private secretaries (confidential assistants to the foreign secretaries), the clerks, including the chief, senior, second-class, and junior, and the précis writers. All of them were mere amanuenses, who came to specialize in certain geographical areas. Of particular interest to historians is the Librarian's Department, the preserve of the Hertslet family, which collected and indexed the official correspondence after 1801 and began to publish the *British and Foreign State Papers* in 1825.



Later chapters deal with ambassadors and other representatives, as well as their posts abroad, whose status reflected the political importance of the country involved. For example, as of 1815, Washington (along with Naples, Lisbon, and some others) was a fourth-class post manned by an envoy extraordinary and minister plenipotentiary. On the fringe of the diplomatic service were the consuls, often merchants whose primary duty was to collect economic information for the government. Such information was normally reported to the consul general, while facts of a political nature were conveyed to the minister. Some Latin American states received a consul general and chargé d'affaires, who was responsible for collecting both types of information.

The work provides considerable interesting trivia, such as the fact that Canning was the only foreign secretary who actually lived in the Foreign Office, as well as appendixes containing biographical information for all officials connected with the Foreign Office during this period, the salaries of various positions, and some stray facts about the Secret Service.

Occasionally the writer passes judgment on the groups of officials he discusses. The under-secretaries were "mediocre" career men; the clerks were "reasonably efficient" during 1782-1821, and true professionals thereafter. The ambassadors and ministers are described as "adequate," and the consuls are said to have performed "tolerably well." By and large, the service was busy, conservative, and class conscious.

This study is based on an impressive quantity of manuscript sources, and a body of secondary works that is somewhat less so. One might disagree with a generalization here or there, but on the whole it is a noncontroversial study, and one that successfully carries out the purpose for which it was intended.

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SEYMOUR DRESCHER. *Econocide: British Slavery in the Era of Abolition*. Pittsburgh: University of Pittsburgh Press. 1977. Pp. xiv, 279. \$14.95.

Historians, both British and American, have repeatedly challenged Eric Williams' thesis that abolition of the British slave trade was a response to the decline in economic significance of the West Indian colonies, a thesis that rested partly on Lowell Ragatz' monumental work, *The Fall of the Planter Class in the British Caribbean, 1763-1833*. While a good bit of scholarly energy was devoted to shredding Williams' *Capitalism and Slavery*, no one took the next logical step of seriously question-

ing whether or not a decline actually occurred prior to the 1807 Abolition Act. Recent studies by Roger Anstey, David B. Davis, Duncan Rice, Howard Temperley, and this reviewer accepted the economic decline theory while refusing to ascribe to it the major credit for abolition. Now Seymour Drescher has undertaken the task of testing that "given," and in his analysis old assumptions fall like ninepins.

In a limited review one can barely list Drescher's major conclusions: 1.) The slave economies, far from declining in significance after 1763, were actually growing in value both absolutely and relatively. Evidence is found in both exports to and imports from the mother country. Coffee and cotton as well as sugar production are included in the analysis. 2.) The British slave trade was not outdistanced by competition in the international market. On the contrary, Britain was gaining the commanding position. A fault in previous measuring was the failure to include increasing amounts of British shipping carried on under flags of other nations, a development partly in response to British regulation after 1799 but also important as a safety precaution during the Napoleonic wars. British capital was invested in the trade of other nations as well. 3.) The supposedly "old, saturated" colonies still had room for economic expansion, were showing a renewed vigor, and were increasing in value along with the newer colonies gained by conquest. Consequently, there was no internal colonial division which forced the older colonies to acquiesce in abolition to prevent slavestocking by their competitors. 4.) The American Revolution was not the turning point in the fortunes of the West Indian empire. War losses of 1775-83 have been emphasized without balancing them against war profits of 1792-1814. 5.) There was not a compensatory shift to India and the eastern empire. 6.) British slavery and the slave trade did not fall because of failure to adjust to burgeoning laissez-faire capitalism. That illusion rests on the misconception that they were sustained by a mercantile policy fashioned to support their rivalry with foreign slave systems. 7.) The British West Indies did not decline because of overproduction. There was only a temporary glut in Britain due to wartime obstacles to transatlantic shipping and to distribution in the continental market. Thus it was the war, not any inherent weakness in the slave system itself, which accounts for temporary distress. 8.) At the end of the Napoleonic wars all economic factors favored a reinstitution of the British slave trade, yet such a policy was never seriously contemplated. Public opinion would not have tolerated it; humanitarian abolition had become a national political issue. 9.) No economic interest can be isolated as responsible for

abolition, nor identified as aligning with humanitarians to effect abolition. In short, the abolitionist attack on the slave trade (1788–1807) came at a time of its greatest profitability and promise. Decline followed abolition.

If Drescher's thesis survives the exacting scrutiny to which it undoubtedly will be subjected, it will indeed stand as a revisionist landmark. But no student of British abolition will be satisfied without two follow-up studies: one which subjects to equally rigorous analysis the humanitarian-political theory of the "cause" of abolition (which *Econocide* seems to validate), and a second which will extend the analysis to the Emancipation Act of 1833.

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F. G. CLARKE. *The Land of Contrarities: British Attitudes to the Australian Colonies, 1828–1855*. Melbourne: Melbourne University Press. 1977. Pp. xiv, 223. \$14.50.

This is essentially a study of British opinion regarding Australia in the formative period of that colonial society. It is a sound and perspicacious analysis and a useful contribution to the connectional history of Britain and the early antipodean settlements. Its success is due to the wide range of views presented with meticulous scholarship. Official (ministerial, departmental, and parliamentary), journalistic, and literary accounts are reviewed, as well as popular views, as contained in ballads and broadsheets—a much-needed corrective, since official documents have loomed so large in appraisals of Australian history until quite recently. This interesting book helps to refute the view held so long that "most Englishmen regarded colonies as valueless sources of international friction and heavy financial burdens to boot."

F. G. Clarke rightly tackles the main contentious issues in his review of British attitudes. The first is the question of convictism. Did the internal interests of the United Kingdom or of the emerging free societies of the Australian penal colonies loom largest in the British mind? The "dilemma" was a serious one, and the quandary of choice between "economy and increased retribution" is well examined here. It was to remain a problem until the 1850s. The crucial constitutional controversies of the 1840–55 period are likewise examined, and the delicate shadings of viewpoint in Britain on the issues have never been so critically investigated. The findings are conclusive: informed, educated opinion in Britain was cognizant of the potential

prospects of Australia, economically and politically, and wished that the imperial connection should be retained. Clarke's treatment of the emigration issue also shows this clearly. Here, and in the section on "Arcady the Brave," there is only one criticism that can be made of the author's method. Debates in Parliament and such literary studies as Samuel Sidney's brilliantly successful *Australian Handbook* of 1848 are drawn upon, but settlers' correspondence is neglected. Exceptionally, there are useful quotations from the Gibbs Papers in the Guildhall Library, London, but these, with the Missionary Papers from which quotations are given, are surely not enough, when one considers the many, many thousands of (extant) letters which were written "both ways" in the period. The views of "ordinary" settlers and emigrants, and of Britons with Australian "connections" who were not otherwise "vocal" (in print) can be discerned from their letters back and forth, and these could have been investigated further.

Still, this study *does* break new ground, in its survey of the periodical press and of the broadsheet literature on key issues. An example is the telling quotation from the minor poet, T. E. Carpenter's "The Emigrant Ship" (1844):

Here in Life's future not one hope I trace,  
There is the land of promise—let us go!

The novelists are looked at too, in detail—Dickens and others, with their picture of Australia as a reception center for Britain's moral cripples and for such inept likeables as Wilkins Micawber. The section on "Arcady the Brave," the view of Australia as a paradise for peasant farmers dispossessed by English industrialism, is a particularly readable and stimulating part of the book. The predilection of Englishmen of the time for an idealized vision of a last agricultural paradise was a main root of the British Romantic movement, and Clarke demonstrates that it was also a mainspring of the emigration and investment urge. The "primitivist" search for a "colonial Arcady" influenced publicists such as John Dunmore Lang, Edward Gibbon Wakefield, Patrick Matthew, and many others, who in turn promoted and advocated emigration. The examination of Wakefield's views (so often referred to—and so seldom understood—by commentators in the past) is perceptive and fresh in approach.

Other vital issues competently handled include the question of whether pastoral industry or crop agriculture should be encouraged. This, of course, would affect the whole social structure of the new society, for either a "squattocracy" or a farming yeomanry would develop, and the population would be either dispersed or concentrated. There were conflicting views as to the nature of the squat-

ter class—their social background and ambitions, as this study shows. Some views on the very small, but extremely influential, class of great landowners who sought to re-create the British pattern of estates with rent-paying tenant farmers (Alexander Berry, James McArthur, and Sir Charles Nicholson are examples) would have been useful additions here.

The shifts of opinion are well charted throughout, and the conclusions are suggestive. For the British upper classes "Australia was a place in which to gain wealth. That accomplished, they would leave. . . . Australia was a mere stepping-stone, a way-station on the road to wealth and retirement in Britain." This sums up very aptly the attitudes of what George Nadel called the "urban gentry" of mid-nineteenth-century Australia, and of their relatives back in England and Scotland, and is only one of the valid, balanced, and useful judgments that emerge from this competent, scholarly study.

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N. P. KLIMENKO. *Kolonial'naia politika Anglii na Dal'nem Vostoke v seredine XIX veka* [England's Colonial Policy in the Far East in the Mid-Nineteenth Century]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 307. 2 r. 16 k.

It is N. P. Klimenko's purpose to show that British "colonial policy" in the Far East, primarily China and Japan in 1856–68, originated in a foreign policy consensus determined by the economic interests of the bourgeoisie. He concentrates on diplomacy, treats the other "Western," "maritime" powers, including the United States, with Britain, and finds in the ample evidence of the pursuit of trade a demonstration of Marxist-Leninist theory.

The book offers little new on several aspects of the subject that have been studied extensively outside of Russia, particularly British policy. Archival materials readily accessible in London and available abroad in large measure in published form were not utilized. Similar limitations affected the research on the other Western powers and China and Japan. Klimenko relied on a few documents in older printed collections and some in Soviet archives, chiefly AVPR. The Soviet documentation is of interest to students of Russian foreign relations. Information drawn from works listed in the brief bibliography, which does include recent foreign monographs (for example by Fairbank and Reischauer) may be welcomed as new by his Soviet audience. But the familiar, stimulating, and to us central, questions arising in studies of British foreign policy and imperialism, international poli-

tics and economics in Asia, and the cultural and other dimensions of Western interaction with China and Japan in this period are missing. Even the issues that troubled Victorian statesmen are reduced to insignificance by the model, in which there are no real alternatives and politics function as in a one-party system. It is not that we would expect Klimenko to answer the questions (to the contrary!), but that he proceeds as if they did not exist.

Most noteworthy is Klimenko's handling of a matter he considers peripheral to international politics in the Far East at the time—Russia's role. The Soviet periodization places Russia on the threshold of developed industrial capitalism, as yet without impetus to share or contest the economic interests of Britain and the advanced Western powers. He states flatly that "Russia did not take part in the actions of the European powers and USA against China." But Russia did in fact play the game of power politics to extract commercial and territorial concessions by the treaties of Aigun, Tientsin, and Peking, and was keenly aware that moves by the Western powers, vis-à-vis Japan as well, affected what were perceived to be important interests in expansion and settlement in Siberia, and particularly in the Amur and Ussuri districts. He avoids many of the considerations that influenced Russian policy and international politics. He also escapes a comparison (such as the one Marx made) between the modes and effects of Russian and British expansion. Curiously, while condemning the Western powers he assigns the defense of Russia's detached impartiality to officials, prominently N. N. Murav'ev ("Amurskii") and N. P. Ignat'ev, who were instrumental in carrying out the very policies he chooses to ignore. Surely he must know that they were reactionary official nationalists and unblushing imperialists whose actions, especially after the Crimean War, were animated by hostility to Britain in Asia.

Klimenko's "consensus" is Soviet, not British. It is a position advanced at the expense of penetrating inquiry and criticism, and of Russian scholarship and knowledge of foreign relations.

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D. A. HAMER. *The Politics of Electoral Pressure: A Study in the History of Victorian Reform Agitations*. Atlantic Highlands, N.J.: Humanities Press. 1977. Pp. x, 386. \$20.00.

D. A. Hamer is well known for his concern with the "faddists" who often troubled the councils of the British Liberal Party. Time and again parliamentary leaders faced challenges from militants

who hawked nostrums, like temperance or disestablishment, which would have rallied the Liberal faithful but alienated moderate voters. Hamer's *Liberal Politics in the Age of Gladstone and Rosebery* (1972) studied this phenomenon from the viewpoint of the Liberal leadership. In the present work Hamer turns to the constituencies and the "electoral strategies" of the militants from 1840 to 1900. It is primarily a study of electoral organization and tactics, as surprisingly little attention is paid to the agitators' political beliefs or goals.

The tactics of four popular pressure groups come under Hamer's lens. The Anti-Corn Law League publicized its cause, registered voters, and interfered in by-elections with such apparent success that it created a model for later agitations. Some of its electoral tactics were tried by the other organizations studied here, especially the Liberation Society, aiming at Anglican disestablishment, and the National Education League, which attacked the Anglican domination of elementary education. The major part of the book, however, concerns the United Kingdom Alliance, whose goal was local prohibition of alcohol. In these pages the Alliance enjoys almost twice the coverage accorded to the disestablishers and education reformers combined, on the grounds that the temperance politicians made more "systematic" efforts to employ electoral pressure in pursuit of their goal (p. 165). This is true in the sense that the Alliance more often faced distasteful choices, such as whether to vote for "the better man" or abstain when no totally committed candidate was on the scene. With the movement's apparent lack of serious impact on Parliament before 1880, however, and the repetitiveness of the issues thereafter, the treatment of the Alliance could have been condensed and the space devoted to other groups.

In a single chapter Hamer discusses the electoral impact of the Irish in Britain, the Chartists, the Reform League, and various working-class groups aiming at labor representation in Parliament. Here the account, in contrast to the rest of the book, is brief and impressionistic, as Hamer is hampered in these cases by the absence of any single long-lived organization around which to center his treatment. By focusing over extended periods on particular *constituencies*, rather than specific organizations, much more could have been done with these groups, especially the Irish.

Hamer uses the minute books and newspapers of both the Alliance and the Liberation Society. Clearly, the tactics of both executives often differed from the naive and often reckless enthusiasm of local advocates. Most commendable is the discussion of the Liberation Society's strategy from 1850 to 1868, which advances beyond all previous work. For the 1870s there is new insight into the

dispersion of the Nonconformist vote by what one strategist called "that modern political evil . . . suburbanism" (p. 144).

Hamer's rigorous approach, concentrating on electoral intervention by the militants, shows that the "faddists" indeed had their calculating sides. However, this emphasis on tactics is overdone. Hamer ruthlessly suppresses the substantive, emotional, and dramatic sides of these reform movements. It is hard to conceive of hot-headed Nonconformists, or even sober ones, being principally concerned with electoral strategy. "Religious liberty" was more important. R. W. Dale looms large in the history of Victorian reform agitations as a brilliant publicist for the causes of Radical Dissent. Here, however, Dale appears in the back room as "a notably crafty politician" (p. 198). This concentration on strategy, to the exclusion of the substantive issues and goals, is hardly the way most of the Victorian reformers in these pages would care to be remembered.

MATTHEW R. TEMMEL  
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CLYDE BINFIELD. *So Down to Prayers: Studies in English Nonconformity, 1780-1920*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. xiv, 296. \$15.00.

Clyde Binfield's is a highly personal book. Its title derives, with characteristic irony, from a comment of his great grandfather, a grocer and Congregationalist deacon, about fellow shopkeepers. They adulterate the sugar with sand and then "go down to prayers" with their families and servants. His great grandfather was not this sort of man and Nonconformists were not this sort of people: "what they stood for was right, and those who failed to share their views could not be quite so right."

In a large sense this book is a defense of Nonconformity in general and of its "golden mean," the Congregationalists, against the aspersions of Matthew Arnold. It is a study of the lives of individuals and families of the Victorian and Edwardian middle class—lives which Binfield argues were decidedly not "so unattractive, so incomplete, so narrow, so far removed from a true and satisfying ideal of human perfection" as Arnold would have us believe.

On the contrary, Nonconformity provided a striking answer to the unbalanced and malfunctioning society of the industrial age. The moral antennae of Nonconformists were acutely attuned to the social ills of society. Moreover, they sensed with particular intensity its fundamental divisions between individual and society, class and community and, most painfully for them, between harmony, "the greatest middle class virtue," and a



religion whose very existence was a sign of a divided society.

These themes emerge most poignantly in the essays on the Baineses of Leeds, "Self Harnessed to the Car of Progress" and on Edward Miall, editor of the militant *Non-Conformist* and the leader of the movement for disestablishment. Edward Baines, Jr. was the most articulate Victorian advocate of voluntarism and self-help. To the six points of the Charter he proposed virtue, industry, religion, sobriety, and education; he passionately opposed government interference of any kind in education. Baines, Jr.'s view of the world and, by extension, that of many others of his class, was formed by the restricted vistas of East Parade Chapel and its equivalent in other cities. By 1870, however, it was clear that the society at large could not be built on the model of a small chapel community. The moral power of the Baineses declined, and some members of the family moved slowly into the Established Church. As for Miall, he was the most prominent example of a Nonconformist minister whose fight against the inequities of a state monopoly of religion helped make England in 1871 a "middle class paradise to perfection." He too, however, had no place to go once the battle for individualized freedoms had been won.

Yet, the book is not about failure. Two of the most original chapters describe the Nonconformist impact on urban and suburban architecture. Both show the impact of architects and designers like Faulkner Armitage and Authur Lee and patrons like Enriquetta Rylands on the making of a mercantile civilization, particularly that "distillation of Victorian civilization," the suburb. Of the final three chapters, "From Non-conformity to Free Churchmanship," the one on the Forward Movement, the missionary alliance which fostered "Christian imperialism," is most rewarding.

This book is too close to its rich and diverse material and too lacking in overall argument to stand as a history of nineteenth-century Nonconformity or even just of Congregationalism. It does not fit comfortably into the general historiography of the subject. Binfield, however, provides much that is new, particularly about women and family life among a stratum of the Victorian middle classes. Most important, he recreates a culture whose impact on nineteenth-century England was inestimable but whose values and integrity have found few modern defenders. *Culture and Anarchy* must be read again through new eyes.

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BRENDA COLLOMS. *Victorian Country Parsons*. Lincoln: University of Nebraska. 1977. Pp. 288. \$11.95.

After a short historical introduction Brenda Colloms devotes thirteen chapters to the life and work of individual country parsons, loosely described as "Victorian" (in fact, several lived well into the twentieth century, one until 1931). Colloms describes her principles of selection as "personal and random," although she asserts that the men she chose represent "certain types or trends within the Anglican Church during the nineteenth century" (p. 33).

There is no doubt that Victorian country parsonages were sometimes havens for men of extraordinary talents and curious eccentricities. In this collection we are introduced to the "Father of British Spiders" (Octavius Pickard-Cambridge), to assorted literary gentlemen, various amateur scientists, one historian, and to a "devotee of hunting, pure and simple" (p. 37). We are told that the well-known clerical diarist Francis Kilvert "had a particular penchant for nubile brunettes" (p. 172) and that Charles Tennyson Turner was an opium addict for a considerable period in his ministry. One clergyman (William Kingsley) rates a chapter chiefly because he lived for a full century, dying more or less in harness.

These sketches do little to illuminate "types or trends" within the Anglican parish ministry in a period (especially after 1830) when that ministry experienced something of a renaissance. Although Colloms does mention the Evangelical and Oxford movements as agencies of revitalization, nowhere does she define the particular contributions these movements made to local parish life in the countryside; nor does she consider other sources of the Victorian pastoral revival (such as church reform by Parliament, alarm at the inroads of Dissent and Methodism, the beginnings of systematic theological and pastoral education, and the developing professionalization of the clergy). She devotes little attention to the ways in which her subjects coped with the demands of rural ministry, and she makes no comparisons at all of their styles as country parsons.

In fact, a good many of Colloms' subjects had little sense of vocation and minimal interest in their professional duties. Pickard-Cambridge was ordained by order of his family and with the purpose of bestowing upon him the family living; in fact, his wife did a good deal of his parish work while he busied himself with his spiders. John Mitford had no religion at all and was ordained "in the entirely reasonable expectation that he would have enough leisure to write novels" (p. 99). R. W. Dixon and J. W. E. Conybeare literally took holy orders for a living, to allow them to pursue interests that brought inadequate financial rewards.

Unfortunately *Victorian Country Parsons* is marred



by errors of fact. Thomas Arnold (who died in 1842) could not have carried "the banner of Christianity against the agnostic forces which followed on Darwin's theories of evolution" (p. 25). Darwin's *Origin of Species* was published in 1859, not in 1858 (p. 112). The school at which Sabine Baring-Gould first taught was Lancing, founded by Nathaniel Woodard, not "Woodward" (p. 239). Leaders of the Oxford Movement claimed that the English church descended from the Apostolic church, not the "Roman one" (p. 25). Charles Simeon, certainly an Evangelical, was hardly a "Wesleyan disciple" (p. 83).

*Victorian Country Parsons* is a collection of intriguing essays about characters who happened to inhabit country rectories. Fascinating as a bedside reader, it contributes very little to understanding the changing roles of village clergy in Victorian England.

BRIAN HEENEY  
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NORMAN MACKENZIE and JEANNE MACKENZIE. *The Fabians*. New York: Simon and Schuster. 1977. Pp. 446. \$12.95.

Like the Webbs, the MacKenzies seem to be "two typewriters clicking as one." The book under review follows, by only a few years, their major study of H. G. Wells and a UNESCO survey of educational learning systems. These will shortly be joined by Norman MacKenzie's edition of the Webb correspondence, in press, and an intellectual biography of Charles Dickens which is already half written. It is a sign of the professionalism of this husband and wife team that such impressive productivity has not been accompanied by a decline in scholarly standards. In their latest book the MacKenzies have made imaginative use of a wide variety of manuscript sources to marshal a wealth of apposite and entertaining quoted matter. On a broad canvas they gradually build up a collective portrait of the first generation of the Fabian Society which is lively and insightful, sympathetic but not uncritical.

The MacKenzies begin with the advantage of an immanently tellable tale. It is true that the "blue-book socialism" on which the Fabians expended enormous intellectual and emotional energies seems as dreary in retrospect as it did to their critics at the time—"the political theory of main drains," as one contemporary novelist caricatured Sidney Webb's drab municipal socialism. But biographically, the Fabian "Old Gang" remain colorful, larger-than-life characters, as close to fiction as to the everyday world of London commerce and public affairs. The Webbs' freakish courtship

(here told brilliantly from the inside, drawing on the unpublished resources of the Passfield Papers) and unlikely marriage partnership; Shaw's antics, equally calamitous with bicycles and women; Annie Besant's passionate oratory; Graham Wallas' rumpled and benign camaraderie; H. G. Wells' incorrigible megalomania—these and a myriad of Fabian idiosyncrasies are paraded before the bemused reader. And where but among this coterie would we expect to find a waistcoated philanderer *par excellence* (married to a writer of children's books) with the Trollopean name of Hubert Bland!

The MacKenzies have chosen to concentrate their attention on the development of the Fabian Society (its name suggested by Frank Podmore) in the period from its foundation in 1884 down to the Great War. What lay beneath the surface of the multifarious tactical and ideological battles which marked these years, the authors argue, was an unresolved tension between those who emphasized the society's role as an agency for enhancing the morality and "character development" of its members and those who wanted it to concern itself more immediately with the reform of society and politics. This internal difference rendered the society less effective than those who, like Ramsay MacDonald, were able to forge concrete understandings between the Labour Representation Committee and the still-ascendant Liberals, paving the way for the creation of the Labour Party.

Previous studies of the Fabians have been of three types: historical-descriptive and institutional (Pease or McBriar); doctrinal (whether sympathetic, as with Fremantle and Cole, or hostile, as with the Muggeridges); or history of ideas and the development of socialism (Letwin, Wolfe, or Pierson). To these the MacKenzies add the perspective of psychohistory. Their book, indeed, bristles with thumbnail psychological sketches. The authors believe that the dominant Fabian personalities seized upon socialism as a faith that would replace the failed intellectual and religious assumptions of their Victorian childhood. "Emotionally impaired by the struggle to liberate themselves", they were lonely figures, with "a sense of difference, expressed at the personal level in pride, in politics by a posture of superiority which made it difficult for them to commit themselves or to work with others as equals" (p. 246). The Fabian fear of too close collaboration with others—whether the Liberal Party, the ILP, or the SDF—was, the MacKenzies suggest, an expression at the group level of the profound anxiety individual members felt about losing their hard-won special identity.

Readers might well feel that by relying on such psychological speculations the MacKenzies have purchased greater narrative effect at the price of

obscuring etiological precision. Psychohistory, it has been well said, explains both too much and too little. Others might find in the authors' concentration on an intellectual biography format too little concern with background historical developments, and certainly such background as is provided is open to the charge of being either too thin or elliptical, or of the romantically sentimental variety. But such reservations address themselves to matters which might fairly be claimed by the authors to fall outside the scope of their undertaking. What the MacKenzies do they do well, and in this work they provide the best overall introduction to the first generation of Fabians now available. Their enthusiasm for the subject is reflected in their inclusion of a number of excellent likenesses of the principal characters in their drama, including a strikingly Bohemian Sidney Webb outfitted in a Jaeger wool suit.

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KENNETH D. BROWN. *John Burns*. (Royal Historical Society Studies in History.) London: The Society. 1977. Pp. 217. \$11.70.

Kenneth Brown presents a well-researched, balanced reappraisal of John Burns in this first major scholarly biography of the early English labor leader. A lecturer at Queen's University of Belfast, Brown is a prolific young historian specializing in pre-World War I labor history. Despite some brief treatments in a few recent books, the standard view of Burns has remained that of a vain, ambitious, incompetent, vindictive betrayer of the labor movement. According to Brown, however, Burns' political career reflected fundamental consistency. He was a radical who attacked privilege, his "socialism" was a reaction to social injustice, and he believed the political process could ameliorate conditions. Championing his own political independence and seeking positive governmental actions, Burns rejected Hardie's approach of building first an ILP and then a Labour Party in favor of cooperation with Liberals on the London County Council, in Parliament, and finally in the Cabinet. Political concepts, more than his vanity, ambition, and rancorous nature, molded Burns' career. These personality traits were also not the main reasons for his failure at the Local Government Board, which Brown attributes to the department's weaknesses, Burns' poor administrative and intellectual abilities, his persistent abhorrence of "wasteful" expenditures, and his antipathy toward the "character faults" of the impoverished.

This is a political biography. While Brown de-

scribes and reinterprets Burns' personality and its effect on his political actions, he steers clear of psychobiography. It is disappointing, however, that Brown's treatment of Burns is confined by an organizational and political approach. Lacking are serious explorations into aspects of Burns' career which pertain to broad economic and sociological questions being addressed by other writers on pre-war working-class history: for instance, Burns' perception of the economic causes of unemployment in the 1880s and Burns' relationship as a "labour leader" and "labour aristocrat" to the Battersea working people—their social and economic aspirations and any developing "class consciousness."

The book is written in a pedestrian manner, is marred by awkward sentences, and fails to capture the excitement and flair of Burns, especially as a young agitator. Nevertheless, it is well organized and cogently developed, and it provides a fine historiographical introduction and an excellent conclusion. This convincing reappraisal of Burns will also aid in the understanding of British politics in general and especially of the organizational and political history of labor in late Victorian and Edwardian England.

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GEORGE HENDRICK. *Henry Salt: Humanitarian Reformer and Man of Letters*. With special assistance of JOHN F. PONTIN. Urbana: University of Illinois Press. 1977. Pp. 228. \$10.00.

George Hendrick's book has rightly brought Henry Salt (1851-1939) to our attention. Salt was a tireless reformer active in such causes as vegetarianism, animal protection, prison reform, pacifism, and socialism. He was a writer and literary critic who wrote two plays, numerous articles, translations, and books, among them studies of Shelley, Tennyson, and Thoreau. And he was a friend of George Bernard Shaw, William Morris, and Edward Carpenter. Unfortunately, this is a thin work offering no new or significant interpretation of Salt, his work, or his times.

This book takes as its thesis a remark by George Bernard Shaw, that Salt "was original and in his way unique" (quoted, p. 6). On the contrary, the real interest Salt holds for historians is that in many ways he exemplifies middle-class British social reformers of the late nineteenth and early twentieth centuries. A list of Salt's causes is an excellent starting-point for a list of humanitarian causes popular during his era. Other well-known reformers who shared some of Salt's enthusiasms were Annie Besant (vegetarianism), Edward Car-

penter (return to the simple life), and Samuel Barnett (opposition to current animal-slaughtering practices): This being the case, it would have been useful had the book attempted to locate Salt in the intellectual and social context of his times and class rather than treating him as an isolated activist. If Salt has a unique position, it is perhaps that he championed a greater number of causes than did some of his contemporaries, or that he took up the particular combination of causes he did. But the author does not deal with either of these possibilities, nor does he make any comparisons between Salt and other reformers of the day.

On the positive side, the book brings forward Salt's hitherto neglected two plays by publishing them in the appendix. They are: *A Lover of Animals*, first published in 1895, and *The Home Secretary's Holiday*, first performed in 1902. Both are vehicles for Salt's humanitarian views and typify his use of literary forms for social causes. Thus, although George Hendrick's book has not supplied an interpretive framework, some of the material in it will nonetheless be of interest to those who study turn of the twentieth century social reform and literary trends.

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F. H. HINSLEY, editor. *British Foreign Policy under Sir Edward Grey*. New York: Cambridge University Press, 1977. Pp. vi, 702, \$57.50.

This volume is the work of twenty scholars, mainly diplomatic historians. The thirty-one chapters deal with a wide range of issues in British foreign policy, and the book's secondary focus on Sir Edward Grey is consistently pursued.

F. H. Hinsley succeeds brilliantly, with the skillful assistance of Zara Steiner, in the massive organizational task of matching up the topics with authoritative scholars, maintaining compatible treatment, and attempting (with partial success) to avoid duplication. Only in the notes does uniformity of style break down. Hinsley, in his half-page preface, explains the *raison d'être* for the book making the curious claim that it can now be written because scholars have "completed the scrutiny of the archives up to 1916." Steiner, who contributed to three chapters, also compiled the exemplary thirty-page bibliography which is marred only by the absence of page numbers in the article citations.

The chapters are organized in four larger segments, starting with a section labeled "Introductory." Keith Robbins, Grey's biographer,

Steiner, who is known for her administrative and diplomatic history of the British Foreign Office, and Clive Parry, an expert on international law, lay the foundation for the following sections which concentrate on specific periods and topics. In the second section K. A. Hamilton's two chapters cover relations with France, and Jonathan Steinberg, D. W. Sweet, and Richard Langhorne do the same for Germany in three chapters. Beryl Williams' chapter and one co-authored by Sweet and Langhorne deal with Russian relations. "Relations with Austria-Hungary and the Balkan States, 1905-1908" by F. R. Bridge opens the study of that crucial area; topical chapters on "The Bosnian Crisis" by Sweet and "The Balkans, 1909-1914" by R. J. Crampton follow. Marian Kent has a chapter on "Constantinople and Asiatic Turkey, 1905-1914"; M. L. Dockrill has one on the Agadir crisis; the late C. J. Lowe writes on the Tripoli wars; Michael Ekstein deals with the Triple Entente before Sarajevo. Shifting to East Asia and America in the next section, E. W. Edwards contributes two chapters, one on China between 1905 and 1911 and another on "China and Japan, 1911-1914." Ian Nish, the historian of the Anglo-Japanese Alliance, writes on Japan in the earlier years and P. A. R. Calvert on British relations with the Americas.

"The Outbreak of the War and the War Years" is the concluding segment with nine chapters. Ekstein and Steiner collaborate on "The Sarajevo Crisis"; Lowe deals with "Italy and the Balkans"; Ekstein completes his contribution with a chapter on "Russia, Constantinople and the Straits." "Asiatic Turkey" by Kent, "Japan and China" by Nish, "Anglo-American Relations" by C. M. Mason, and "The Blockade" by Arthur Marden are further topical chapters. A chapter by Steiner on the wartime Foreign Office and another by Robbins on "Foreign Policy, Government Structure and Public Opinion," dealing with personalities, governmental organization, and administrative history, complete the volume.

While this book will not be the last word on foreign policy under Grey, so many geographical and topical units are examined rigorously that to ask for more may be excessively critical. Still, areas such as Southeast Asia and East Africa do not receive even the cursory treatment accorded the Americas. With more evidence, Steiner and Robbins repeat their earlier contention that Grey personally directed foreign policy without being guided by the anti-German clique in the Foreign Office. Grey's determination to maintain the entente with France and later with Russia despite the souring effect on dealings with Germany and Austria-Hungary is restated by several of the authors. Langhorne shows evidence that claims of

fundamental improvement of Anglo-German relations between 1911 and 1914 was a myth. Once the war began, Steiner believes that Grey made a mistake in allowing the service chiefs to dominate foreign policy decisions in the Cabinet without objection, but Grey's competence is criticized most severely by Kent in her account of British policy in the wartime Middle East. She demonstrates that Grey did not understand negotiations there for which he was ultimately responsible, and she believes he "was not the right man for the job" (p. 451).

Eyre Crowe's reputation as an able Foreign Office official is confirmed. But Steiner points out that his influence on Grey has been exaggerated, and she explains why. Crowe was a prolific memo writer, in part because he had little direct access to Grey and often could have his views considered only when written out. His memos and minutes have provided an extensive record of his views, and he keeps reappearing in this book. Charles Hardinge, Arthur Nicolson, William Tyrrell, Francis Bertie, Richard Haldane, and Lord Robert Cecil are also cited frequently but not as often as Crowe.

The study of British foreign policy is well served by this group of scholars who have produced a volume with diverse but sharply focused parts. Steiner's chapters on the Foreign Office before the war and, with Ekstein, on the July crisis are outstanding. Steinberg wrote a fine chapter on the naval race in spite of its misleading and generalized title, "The German background to Anglo-German relations, 1905-1914." R. J. Crampton's chapter on the Balkans is a model for detail and precision in historical writing and documentation. Kent and Edwards argue persuasively that political interests and military strategy took precedence over economic interests in foreign-policy decisions.

This book will be an indispensable tool for all diplomatic historians working in this period, and it should stimulate their scholarship. Zara Steiner, who is so prominently involved with this work, is now publishing a volume on *Britain and the Origins of the First World War* which promises to add to her outstanding reputation in the history of British foreign policy. Previously unknown sources are adding to the potential for more such scholarship. For example, who knows what important information to alter our understanding of Anglo-Russian relations will be found in the recently discovered papers of E. J. Dillon, the able and influential correspondent in Russia for the *Daily Telegraph*?

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HELEN FEIN. *Imperial Crime and Punishment: The Massacre at Jallianwala Bagh and British Judgement, 1919-*

1920. Honolulu: The University Press of Hawaii. 1977. Pp. xv, 250. \$12.50.

On April 13, 1919 General R. E. H. Dyer faced several thousand unarmed Indians in an enclosed area of waste land at Amritsar. The general thought he was faced with rebellion and made up his mind to keep the revolt from spreading by shooting immediately. His purpose was to terrify the population into inactivity. Dyer calculated that between two and three hundred were killed. Investigation by the government and the Allahabad Social Service League discovered about 379 dead and probably three times as many wounded. A Congress Commission found nearly 1,200 dead and 3,600 wounded. An intelligent guess would point somewhere between the official figures and the Congress figures.

No doubt Dyer had an iron but misconceived sense of duty, yet his reaction was almost predictable. He was a veritable avatar of the colonial society which created him. He left his native Punjab only to be educated at Cork and Sandhurst. Steeped in the Mutiny tradition (India must be held by force) and handicapped by a soldier's inclination to find a solution in military action, he jumped to a false conclusion. His action precipitated a nationalist revolution rather than stopping one.

Helen Fein has analyzed the historical evidence of this incident on a class-based model. She concludes that a member of a superior, homogeneous class who commits an immoral act to support the dominant group will be legally justified by the class in power. In this case that was the attitude of the sahibs and the Tory Party. Dyer was caught between the upper and nether millstones of high-level policy—Churchill was anxious to bring off the Indian reforms of 1919—and was retired from the army, but he was subsequently the recipient of a large purse got together by a sympathetic conservative group.

I have only two criticisms, and they do not invalidate Fein's thesis. First, members of a lower class may be loyal to a dominant group whether or not their natural ability (a debatable concept in any case) is frustrated. the old Indian sudra caste was stable because of a belief in karma. The medieval European peasant class was stable because of a faith in the estate system. And the contemporary United States workers are stable because of a belief in the possibility of upward social mobility.

Second, there is the question of distance in time. It is difficult to analyze the Watts riot of 1965 in order to predict. And the further one goes back in time, the more difficult it is to nail down the data.

On the whole, however, Fein has contrived a model which is accurate for the imperial situation.



This volume ranks far above the run-of-the-mill study both in importance and execution.

MARK NAIDIS  
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BEN PIMLOTT. *Labour and the Left in the 1930s*. New York: Cambridge University Press. 1977. Pp. xi, 259. \$13.95.

The history of labor politics in Britain during the 1930s is becoming a matter of considerable debate and controversy among professional historians. Ben Pimlott's book is only in some degree a contribution to this discussion, since, while he sets out certain parts of the story with care, he never engages in a detailed consideration of the main questions involved.

His thesis is the simple one that the Left or the Labour Left (Pimlott uses the terms almost indiscriminately) during the years of the thirties "was consistently wrong on tactics" (p. 5) and that in terms of political effectiveness "the Labour Left scores very poorly indeed" (p. 6). His book has twenty quite short chapters divided into four main sections, each with a unifying theme. The first, "Labour and the Crisis," deals with the Parliamentary Labour Party (PLP) in the aftermath of the 1931 crisis, the disaffiliation of the ILP, and the emergence of the Socialist League as the main focus of dissent within the official movement. Pimlott brings out very well the importance of the national executive committee vis-à-vis the PLP but seriously underplays the significance of what, in 1934, became the National Council of Labour in which the right-wing trade unionists were dominant. In this last matter Pimlott is in disagreement with most commentators on this period, including D. E. McHenry, the American contemporary who wrote an interesting book not listed in the bibliography, and Henry Pelling, upon whose other judgments Pimlott relies rather heavily. Pimlott is usually careful in his statements, but occasionally he produces an incautious generalization, as when he describes J. A. Hobson as "the great economist and exponent of under-consumptionist doctrine whose disciples included Lenin" (p. 61). The second section, "United Front," describes the background to the growing involvement of the Communist Party with the Labour Party, the development of the united front tactic, and the history of the unsuccessful Unity Campaign of 1937. The third section tells a story which does not appear in such great detail elsewhere and which, although it is quite a minor episode in the 1930s, is the only original and new material that Pimlott presents. This records the growth of discontent among the Constituency Labour Parties regarding their treatment by the leadership, especially at Annual Con-

ferences, and their victory in 1937 when they were given seven, instead of five, representatives on the national executive and were allowed to elect them independently of other groups. The four chapters in this section are given the slick, meaningless headings of "Repression," "Revolt," "Struggle," and "Revolution"; and apparently Pimlott really does believe that this constitutional revision represented a structural change of revolutionary proportions. He admits, however, that its impact was not seriously felt until the emergence of the Bevanite opposition after 1951. The fourth section deals with the campaigns for a Popular Front toward the end of the decade, and the internal conflicts which this generated. It includes a chapter on the Left Book Club and ends with a brief survey of the attitude of the labor movement to the outbreak of war.

Pimlott offers what it is not unfair to describe as the official history of the dissident Left groups. Indeed, his book can be understood as an extended gloss upon the published and unpublished diaries of Hugh Dalton. For the lay reader the arrangement of the work must be confusing, for its structure obscures anything approaching a chronology of these years. The specialist has other problems. Pimlott writes lucidly and his research is adequate if narrowly focused; but in the concluding chapter, which sums up his thesis, he offers only assertions and not arguments. We have been given a coherent account of the internal evolution of certain political campaigns and movements; what we are never told is why the Left was in continuous opposition since there is almost no indication of the attitudes, policies, and actions of the dominant leadership of the Labour Party and the TUC. There were, as we all know, fundamental problems confronting the British labor movement in the 1930s—above all, unemployment, fascism, appeasement, and Spain—but what the Labour Party leadership did or did not do on these matters is not Pimlott's concern. We cannot comprehend why the Left behaved in this or that way, because their motivations, arguments, and debates are never explained. Throughout the decade the Left was confronted with an establishment, political and industrial, which never lost a crucial vote in any Annual Conference during the whole period under review; and the reactions of the Left—sometimes flawed and ill-judged and nearly always unsuccessful in the short run—can only be analyzed in terms of their minority situation. This Pimlott does not seem to understand, and while his book will be of some use to scholars—it is, among other things, a handy compendium of facts—it is far from providing a substantial contribution to the growing literature on this fascinating decade.

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TOM FORESTER. *The British Labour Party and the Working Class*. New York: Holmes and Meier. 1976. Pp. x, 166. \$11.50.

Tom Forester's study is intended for the general reader, academic and nonacademic; it draws together much of the secondary literature on the Labour Party into a general interpretation of its development. But the author also deals with major questions in the life of the party. He is particularly concerned with the Labour Party's relationship to the working class, one third of which continues to support the Conservatives. His study is also informed by a desire to refute the argument, advanced by Barry Hindess, *The Decline of Working Class Politics* (1971), that the British Labour Party has been undergoing a process of "deradicalization" and increasing control by the middle class.

As an introductory account of the development, the structure, and the present position of the Labour Party the book serves its readers quite well. It is clearly organized and well written, and Forester's assessment of the party's growth, despite his obvious attachment to the labor cause, is judicious. Moreover, through his use of recent research on voting behavior, the author provides an understanding of the activities of the party at the local level not often found in books of such limited scope. He does not, however, throw light on the tensions within the Labour Party; he is not concerned with the ways in which ideological differences or the special position of the trade unions have affected the development of the party.

Forester presents a plausible though not conclusive case against the argument that the Labour Party's relationship to the working class has been changing in recent years. Much of this argument, he maintains, rests on the myth of a "golden past" when the party's dedication to socialism was much stronger than at present. He argues, quite correctly in my view, that the place of socialist ideology in the Labour Party has not altered significantly. Forester also rejects, partly on the basis of his own research into party activity at the constituency level, the view that the middle class is taking over the party. Middle-class elements played important roles in the party from the beginning and, at the local level at least, that influence has not increased markedly. Forester does concede that persons of working-class background are less evident among the Labour Party M.P.'s than formerly, and he attributes this development to the increasing professionalization and sophistication of politics. For the leading positions in the party the workers are "more disadvantaged" than ever.

Forester ends his study of the Labour Party on an optimistic note. Having rejected the view that working-class politics in Britain has entered a time

of decline, he concludes that the labor movement remains a strong force "for social—and socialist—change."

STANLEY PIERSON  
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T. RYLE DWYER. *Irish Neutrality and the USA: 1939-47*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. lxi, 241. \$16.50.

The underlying thesis of T. Ryle Dwyer's examination of Irish neutrality during World War II is that given the substantial internal pressures on Eamon de Valera and de Valera's own view of Ireland's self-interests there was no alternative to neutrality. While personally opposed to the Nazi regime, de Valera had to be as evenhanded as possible in public even while making some friendly gestures to Britain clandestinely.

In arguing this thesis from de Valera's point of view Dwyer is on the whole successful, although wider problems with this book detract from the presentation of what is presumably the central argument. A major flaw is that Dwyer does not stay long enough with any one theme, giving the book a rambling tone. A number of themes are available: Irish policy as seen from Dublin; the conflicting interpretations of national self-interest by de Valera and United States Minister David Gray, who is successfully portrayed as the major antagonist to de Valera's protagonist; and Anglo-American relations as affected by Ireland. All of these are touched on but none is fully developed.

Moreover, in trying to make concern for Ireland appear as a major factor in United States policy, Dwyer distorts his presentation and actually undercuts this theme on several occasions. For all of the influence of Irish-Americans within the Democratic Party and in Congress, it was never sufficient to cause President Roosevelt to alter his policies, as Dwyer himself admits (p. 41). Nor was Ireland ever allowed to get in the way of the greater common purpose seen by the political leaders in Britain and the United States. Ireland, as a potential problem in Anglo-American relations, loomed large in David Gray's mind, but as Dwyer shows, the American minister often acted very much as a free agent. Gray's initiatives could not always be identified with American policy in Washington.

While adequately researched, the work is further marred by inadequate footnoting, which leaves several key observations wholly unsubstantiated. Dwyer's treatment of the development of United States policy toward the war is simplistic and trite, and his mention of the CIA four times is gratuitous. At the end of the book there is a collection of biographical sketches of leading pertinent states-

men, that contains much extraneous information for several entries and does not add to the work. This section is mistakenly labelled a "Glossary."

While not presenting any new information, Dwyer has made a plausible case for de Valera's firm stand on Irish neutrality. This might have been better treated in a full-length article, which would have required Dwyer to select one theme and to present only the evidence relevant to his argument.

MARK M. LOWENTHAL  
*Library of Congress*

DOUGLAS CLARK BAXTER. *Servants of the Sword: French Intendants of the Army, 1630-70*. Urbana: University of Illinois Press. 1976. Pp. xvii, 243. \$11.95.

Despite the loving attention given to administrative history by scholars of seventeenth-century France, the institution of the army intendency has escaped the attention of all but a few specialized *dévots*. In a useful and interesting synthesis, however, Douglas C. Baxter has brought the army intendency into historical focus.

Baxter examines the functions of the army intendants as well as their social origins and connections. He begins his study in the 1630s, when France entered the Thirty Years' War and the army intendency received a new impetus. The terminal date is 1670, by which time, the author contends, the army intendants had acquired the essential characteristics they would retain for the entire seventeenth century, if not for the duration of the Old Regime. The original research consists essentially of a reading of some two hundred manuscript volumes in the Archives de la Guerre and a consultation of the genealogical volumes in the *cabinet des titres* at the Bibliothèque Nationale.

With regard to function, Baxter takes pains to distinguish the army intendants from their more famous colleagues, the provincial intendants. This is not altogether easy to do, given the interchangeable nature of the two functions in the 1630s; but the differences emerge clearly enough. The commissions of army intendants were written for a campaign year and had to be renewed annually, contrasting with the implicitly longer tenure of the provincials. Moreover, army intendants tended to serve outside France, thereby forfeiting the territorial base of the provincial intendants. For these reasons, army intendants were even more dependent upon the crown and responsive to its wishes; they played an important part in the strengthening of royal authority.

Equally, if not more important, the author detects a "fundamental change" occurring between the 1630s and the 1660s, as the judicial functions of the army intendants declined and they became

administrative, financial, and governmental officials. At the same time, they fell once and for all into the grip of the secretary of state for war, who made them the essential link in new lines of administration running from the ministry at the top to the *commissaires des guerres* at the bottom.

Corresponding to these administrative changes was a transformation in the social character of the army intendants. At the beginning of the period, these officials tended to represent the third generation of upwardly mobile bourgeois families, and they had strong roots in the *noblesse de robe*. By the 1660s, however, "new men" appeared, as Le Tellier and Louvois created a patronage system that drew upon former *commissaires des guerres* who had proved themselves in the royal service. Proven administrative skills became more important than judicial education and robe experience.

Although there is much else of interest in Baxter's study, a few suggestions are in order. The statistical tables in chapter two depicting the social background of the army intendants would have been more effective had the author specified the relationship between the subgroup for which statistics are available and the group as a whole. Moreover, the author's information about the private wealth of his officials is random and unsystematic (pp. 22-29). There are interesting glimpses into the ways the crown rewarded its favored intendants financially, and one wishes that this subject could have been pursued.

But these are small points. We are indebted to Baxter for a solid study that will be widely consulted in the future.

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MARK HULLIUNG. *Montesquieu and the Old Regime*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xi, 258. \$11.50.

Mark Hulliung claims that historians foolishly have reduced Montesquieu to a defender of aristocratic privilege. He should read more historians. Hulliung believes that he has discovered a Montesquieu of remarkably incisive intellect, a thinker who eliminated "essentialist" categories from Aristotle's political science and offered a dynamic, historical model of feudalism (based upon the identification of feudalism with the inheritability of the fief). Using that model, Hulliung's Montesquieu was able to comprehend the historical and moral consequences of both Eastern and Western despotism, refute Machiavelli and his disciples, accurately define the ills and limitations of "the Old Regime," and identify Walpole's England as a "radical" new model for the happier future of the species.

As history, *Montesquieu and the Old Regime* is idealist and anachronistic, removing Montesquieu's thought from its social, political, and intellectual context and placing it in dialogue with the classics of "political science" and "political sociology" from Aristotle to Machiavelli to Weber and more modern theorists of the nature and etiology of the bureaucratic state. The Montesquieu of the *Spicilège*, of intellectual friendships with the idiosyncratic figures of the *Académie des Inscriptions et des Belles-Lettres*, of the audience of *The Turkish Spy*, of Regency Paris, and of the *parlement de Bordeaux* is simply lost from view. As a sustained essay on one possible reading of Montesquieu within the context of Western grapplings with the problems of political legitimacy and justification, the nature and proper division of power, and the historical dynamic of authority and subservience, however, Hulliung's work, despite many a muddle and flaw, is a provocative and at times quite rewarding reflection on the history of political thought.

Within his own framework Hulliung is not consistent. He cannot have (as he wishes to have) a Montesquieu who correctly identifies England as the model of a "post-feudal," "new regime" state and who correctly identifies wars of colonial aggrandizement as the very essence of eighteenth-century "old regime" despotism. This, however, is one of the work's least significant flaws as history. First, Hulliung is never quite able to explain what he (let alone Montesquieu, who never used the term) might mean by "old regime." Furthermore, Hulliung fails to distinguish between Montesquieu's contempt for courtiers and his far more complex attitude toward the aristocracy as a corporate entity. He employs terms such as "capitalist," "democratic," "radical," and "republican" to explicate Montesquieu's vision of England, without ever defining their meaning beyond Montesquieu's admiration of a nation with high levels of commercial activity and institutions of political representation. Most disturbingly, Hulliung translates Montesquieu's language into modern idioms that are wholly inappropriate to the conceptual and linguistic context of Montesquieu's work. Thus: "To someone of Montesquieu's temperament, civic virtue is actually much more awesome than the categorical imperative" (p. 30); "consumption and demand were along class rather than mass lines" (p. 51); "Eastern Leviathans are paper tigers. . . ." (p. 180); "skipping over the *laissez faire* of the nineteenth century, Montesquieu embraces Keynes. . ." (p. 197); "in one fell swoop, he hoped, the bourgeois values of the new republicanism would do away with feudal valor. . ." (p. 225); "the *Esprit des Lois* agreed with [Christopher] Hill's contention that 'the plunder of the

Church . . . stimulated the development of capitalism in England' " (p. 227). Readers also will be surprised to discover a Montesquieu who allegedly explains phenomena in terms of "charisma" and "class stalemate" (p. 35).

In terms of certain traditional problems of Montesquieu scholarship, Hulliung is quite astute in his analysis of his subject's view of the relationship between medieval crown and nobility and in his focus on Montesquieu's complex analysis of the venality of offices. He is persuasive (and in agreement with almost everyone who has written on the subject) about the reality of Montesquieu's cultural relativism, but he does not answer the more difficult question of where this relativism ends and Montesquieu's belief in natural law begins.

In brief, Hulliung's is not an uninteresting mind to engage. For those, however, who wish to encounter Montesquieu's mind and to learn something of the world from which he emerged, this is not the work to read.

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URSULA HELMICH. *Arbeitskämpfe in Frankreich: Ein Beitrag zur Sozial- und Rechtsgeschichte, 1789-1939*. Foreword by IRING FETSCHER. Meisenheim: Anton Hain. 1977. Pp. ix, 351.

In the nineteenth century, organized French workers scarcely bothered to conceal their scorn for politicians, judges, and administrators. When the programs of associationalists, cooperative socialists, and anarchosyndicalists did not call for a drastic reduction in governmental power, it was only because they planned to do away with the state altogether. Whenever possible, worker organizations tried to keep their contact with civil authority to a minimum; unfortunately, the administration refused to cooperate. On occasion it repressed the workers' movement, but always and everywhere its officials carefully observed and closely regulated strikes, trade-union organizations, and collective actions.

Ursula Helmich's book combines the works of legal and labor historians in order to examine the relationship between the law and working-class protest. While Helmich's account of the developing labor movement in the years between 1789 and 1939 is based on a thorough study of secondary sources, the discussion of changing legal attitudes relies largely on her own analysis of legal decisions and parliamentary records. Each chapter of the book summarizes the major industrial trends of a period, describes the growth of worker and employer attitudes and organizations, and discusses how the new problems posed by industrial growth and the pressures of different social groups af-

fect executive actions and legislative and judicial decisions. Major laws and legal interpretations are condensed succinctly and explained clearly.

Helmich finds that a variety of forces influenced legal restrictions on trade unions and strike actions and that the attitudes of executive, legislative, and judicial authorities changed at very different rates. In the early nineteenth century industrialists used all manner of economic and philosophic pretexts to persuade authorities to prohibit trade unions. At the same time, employers' organizations which were in principle subject to identical objections flourished. It was the political pressures of plebiscitary democracy which finally forced Napoleon III to partially legalize strikes. Even after trade unions were authorized in 1884, guerrilla warfare against both unions and strikes was still being carried out by the courts where conservative, corporatist ideas prevailed. Although criminal penalties had been taken away by the legislature, stiff civil penalties continued to be imposed. It was not until the Popular Front established its own labor court, the *Cour Supérieure d'Arbitrage*, that corporatist ideas began to die away.

The topic that Helmich has selected is a fascinating one and in need of synthetic treatment. Unfortunately, Helmich's preoccupation with national-level political debates, appellate court decisions, and political histories of the trade-union movement have led her to ignore the actual circumstances of law enforcement. What sanctions were most often used against strikes and what agencies enforced them? How often were civil suits filed against trade unions? Helmich does not discuss at all the army of police spies who were partly used for surveillance but, after the turn of the century, were oriented increasingly toward disruption and provocation. All these questions lead to another. French laws restricted the kinds of property unions could own, determined how they might spend their dues, and made their treasuries readily subject to civil suits. To what extent did such laws contribute to the weak organizational structure of French unionism? It is possible that the unbureaucratic, decentralized structures which revolutionaries counterposed to centralized government were in fact creations of state power.

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RENÉ ALBRECHT-CARRIÉ. *Adolphe Thiers, or the Triumph of the Bourgeoisie*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1977. Pp. 176. \$9.95.

René Albrecht-Carrié has written a derivative, succinct biographical essay describing Adolphe

Thiers (1797-1877) as a bourgeois politician who remained committed to the principles of 1789 while accommodating himself to the vicissitudes of French national developments. Upholding the conventional opinion that the bourgeoisie gained predominance in the wake of the Revolution, the author qualifies that interpretation with the observation that France remained a nation of peasants and artisans. He credits Thiers' remarkable performance to a capacity for work and an ability to use information.

A trained lawyer from Marseilles and initially a liberal journalist in Paris, Thiers participated as legislator and administrator in three regimes. He consistently showed concern for proper governance, which to him meant careers open to the politically talented and rule by the financially qualified. A staunch supporter of bourgeois prerogatives and a dedicated proponent of social order, he handled matters of state responsibility for domestic tranquility, sound finance and free enterprise, and the national interest in foreign affairs. Yet he never sponsored social reform, formulated domestic policies, or initiated involvement abroad. Always pragmatic, he deliberately maneuvered within the practicalities of political trends, erroneously during the 1840 Near East crisis but presciently on the eve of the Third Republic. Not deflected by what appeared to him to be irrelevant ideological considerations, he set aside his preference for a monarchy in order to accept the presidency of the Third Republic.

All Thiers' biographers have judged his response to the Paris Commune and to the task of national reconstruction after the Franco-Prussian War as the most significant events in his career. Albrecht-Carrié asserts that Thiers' crushing of the Communards perpetuated the social cleavage between the mass and revolutionary leadership that has characterized France from the Revolution to the present. He credits France's swift recovery after executive error and military defeat to Thiers' conservative reorganization of the government in the hands of bourgeois functionaries.

Through judicious emphasis and subordination of complexities in France and abroad, Albrecht-Carrié sets in relief Thiers' multiple activities, thereby contributing to readers' understanding of the forces that move a society through its political institutions. Written with verve in the centennial of Thiers' death for an educated American audience, this compact volume contains deliberate reference to issues which engaged Thiers as well as us: division of power between executive and other government agencies, *raison d'état* as a cover for partisan operations.

According to Adolphe Thiers, the essential qual-



ity that must distinguish a historian is understanding of human affairs. Albrecht-Carrié displays this requisite in his biographical essay.

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THEODORE ZELDIN. *France, 1848-1945*. Volume 2, *Intellect, Taste and Anxiety*. (Oxford History of Modern Europe.) Oxford: Clarendon Press. 1977. Pp. 1,202. \$29.95.

The book under review is the second volume of Theodore Zeldin's study of modern France for the Oxford History of Modern Europe. In the first massive volume *Ambition, Love and Politics* (1973), Zeldin analyzed the varieties of bourgeois careerism, the unifying emotional effects of middle-class marriage and family life, and the colorful spectrum of political expressions in the Second Empire and Third Republic. In the present even more massive volume he plunges into investigation of the mental culture, the tastes, and the anxieties of Frenchmen and Frenchwomen living between 1848 and 1945.

It is Zeldin's belief that the historian stands to gain greater insight into France by studying the forces and attitudes that united the French rather than the "usual" exploration of the issues that divided them. An approach as purportedly unconventional as this requires appropriately unconventional investigative methods, and, as Zeldin finally describes them in his conclusion, these are more artistic than historical. Thus he is engaged in "a kind of *pointillisme*" and a cubistic "portrait of the individual simultaneously from several different sides, as though I were painting not just the obvious face, but the back of the head also, and the features rearranged so that they may all be seen at once." He contrasts this method of "juxtaposition" with that of causal analysis which he has forsworn here, since "to talk of causes means to talk of proof and it is difficult to prove motives, character or interpretations" (p. 1,157). His distaste for traditional techniques extends even to the "tyranny" of chronology. Accordingly, there is no provision for *development* within each of the book's major sections; descriptions of attitudes or behaviors must often do equal service for both the 1850s and the 1940s. As if to underline the book's anti-chronological bias Zeldin regularly cites the results of *sondages* taken in the 1950s and 1960s as insights into nineteenth-century opinion. The book's strengths and weaknesses more or less flow from these methodological crotchets, though Zeldin breaks his own rules often enough to allow his readers an occasional glimpse of the forest through his brilliantly rendered three-dimensional trees.

Far and away the most effective sections of the book are the chapters on the culture of education. Zeldin presents "educationism" as the intellectual elite's alternative ethic to "industrialism," a mode of self-assertion and self-validation which allowed this caste to tyrannize over the national literature and language in a way possibly unique to Western Europe. In a more systematic manner than elsewhere in the book, Zeldin extends his analysis to educational structures themselves, to the texts they used, the examinations they gave, and the corporate mentalities of their faculties and their students. Despite their different political labels, the educated classes had more in common with one another by virtue of a certain cognitive style than with other Frenchmen. It was this preoccupation with the forms and modes of expression, he argues, that was at once the greatest glory of French culture and the best explanation for its lamentable failures in the realm of material culture.

In the matter of general culture Zeldin explores the changing fortunes of taste in fashion and the arts, humor, popular literature, the press, applied technology, and eating and drinking. Throughout these sections, however, Zeldin advances the doubtful notion that variations in taste are generally unrelated to changes in social or economic structures or to any new or demonstrable needs. He prefers to argue that taste follows its own internal laws of development and is propagated mechanically from social superiors to social inferiors. Elsewhere Zeldin explores the abundant medical literature of the day for clues about the fears and emotional problems of contemporaries, and examines the colonial, military, and criminal classes for insights into the relationship of authority structures and violence. His conclusions here are decidedly superficial, however; he says only that Frenchmen in hierarchical structures suffered a certain "mutual incomprehension" (p. 947).

Apart from some marvelously deft characterizations and a surpassingly clear style, Zeldin's history of the individual and his attitudes is not much more than a history of some celebrated personages whose attitudes are unlikely to have been typical of others. The lives of these individuals are presented throughout the book in long strings of mini-biographies and anecdotes summarized from secondary sources. Such unanalytical exercises ultimately explain little about the relations of material structures and their cultural expressions. If we are to understand the individuals who inhabit these structures as anything more than elaborate reflexes, we must see them shaping their culture in response to problems they face in some real world. The central shortcoming of this vast synthesis is not merely a failure to satisfy the historical special-



ist, which it shares with all other syntheses, but the unfortunate effect of a clear choice to write history without causes.

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FÉLIX-PAUL CODACCIONI. *De l'inégalité sociale dans une grande ville industrielle: Le drame de Lille de 1850 à 1914*. Lille: Université de Lille III. 1976. Pp. vi, 444. 50 fr.

This contribution to the social history of industrial society is an abridged version of a doctoral thesis presented at the University of Lille in 1971. Using the *archives de l'enregistrement* of Lille, the author has constructed cross-sectional views of the city's wealth, as reflected in the wills of its citizens, during four periods of time: 1856-58, 1873-75, 1891-93, and 1908-10. His aggregate data show that in the half-century during which Lille became one of Europe's great industrial centers the upper classes (*les classes dirigeantes*) accounted for 8.94 percent of the deaths and 90.6 percent of the inherited wealth; for the middle classes (*les classes moyennes*) the percentages are 28.34 and 9.07, and for the mass of *lilloises* (*les classes populaires*) the percentages are 62.69 and 0.26 respectively.

The "drama" which gives the book its subtitle concerns fluctuations in the ratio of wealth between rich and poor. In the middle of the nineteenth century the mean value of the estate of a deceased *industriel* of Lille was 758,862 francs and that of a worker was 78 francs (a ratio of 9,728 to one); shortly after the fall of the Second Empire the mean values were 1,248,875 and 61 (a ratio of 20,473 to one); in the *Belle Époque* they were 1,396,823 and 68 (a ratio of 20,541 to one); and on the eve of the Great War they were 2,023,443 and 207 (a ratio of 9,775 to one). These figures are staggering! While supporting the familiar contention that the condition of France's working class improved in the decade or so before 1914, they also suggest the degree to which vast social inequality had become part of its daily life and death (a structure which, not incidentally, persists to this day).

Specialists in the use of quantitative methods will appreciate how Codaccioni has refined and expanded the research techniques introduced by his mentor, Adeline Daumard, in her work on the Parisian bourgeoisie, and also the fact that he has extended the project of *codification socio-professionnel* into the twentieth century. Furthermore, he is more sensitive than many others have been to the human context of social structure, which might remain invisible if one depended on numbers

alone. For example: a ninety-two-year-old widow dies in 1910, leaving only 120 francs worth of property behind her. Should she be classified as belonging to the lowest ranks of the working class? No, as a *propriétaire* because she lived with her son, a physician with an office on the wealthy *rue Royale*.

Having ably established the dimensions of social inequality in Lille, however, Codaccioni does not tell us enough about the implications of his findings. I was disappointed by the chapter on the election of Paul Lafargue to the Chamber of Deputies in 1891, intended as an *essai d'étude territoriale*, and think that the author missed an important opportunity by offering only fleeting comparisons between patterns of worker residency and labor militancy in Lille and other French cities. The failure to address large historical questions, if only indirectly, means that Codaccioni's work has less value for American historians of Europe than do the recent theses of Maurice Garden and Michelle Perrot.

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MARTIN BLUMENSON. *The Vildé Affair: Beginnings of the French Resistance*. Boston: Houghton Mifflin. 1977. Pp. 287. \$10.00.

In this work on the "beginnings of the French Resistance," Martin Blumenson tells the story of a small group of French patriots, men and women, who, from the earliest days of the occupation in 1940, undertook to resist the Germans. Their leader was Boris Vildé, a linguist at the *Musée de l'Homme* in Paris, and under his direction, according to the author, a far-flung network of resistance developed in France even before Hitler's attack on the Soviet Union in 1941. Based on the *Musée de l'Homme*, the group engaged in a variety of activities: aiding allied soldiers to escape to England, gathering intelligence for the British and De Gaulle, distributing tracts against the Germans and Vichy, and, briefly, publishing a newspaper called *Résistance*.

Apparently betrayed by one of their number, most of the group's members were rounded up by the Germans early in 1941 and were later tried and condemned, most of them, to prison or to death. Vildé and seven others were executed early in 1942, but not before they and their comrades had "helped to bring France from the stupor of defeat, the panic of the exodus, the disillusionment of the German conquest, the paralysis of the national will to a reawakening of morality and dignity and hope."

In preparing his book, Blumenson had access to

papers collected by Claude Aveline and Germaine Tillion and to the materials assembled under the direction of Henri Michel, dean of Resistance studies. He also interviewed the survivors and the friends and relatives of these and others active in the group. He relies, too, on notebooks kept by Vildé and Pierre Walter while in prison, although these are not mentioned in his "Author's Note." He does not use footnotes.

In page after page Blumenson records conversations in marvelous detail or provides the most minute sort of information about the feelings, thoughts, demeanor, and gestures of the participants as they lived the events he describes. Take, for example, this passage where Jacqueline Bordelet asks Yvonne Oddon to be allowed to participate in the "other [Resistance] work" being done by the *Musée de l'Homme* group:

After a long moment of silence, Yvonne said, "No, my child, you are too young."

"I am twenty-seven," she cried. But she knew she looked eighteen.

Yvonne shook her head. "No," she said. "You must never ask again or say anything."

"Please," she begged.

Yvonne shook her head and turned away.

Blumenson assures us in his preface that all of this is "real," but I find it hard to believe that such details could have been so meticulously recorded at the time or so well recalled by the participants over thirty years after the events described took place. The effect of such passages, and there are many of them, is to detract from the verisimilitude of Blumenson's account, not add to it.

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RAYMOND CARR. *The Spanish Tragedy: The Civil War in Perspective*. London: Weidenfeld and Nicolson. 1977. Pp. xvii, 336. £10.00.

Spain, even more than other countries of Western Europe, has undergone a number of radical governmental changes in this century. From the constitutional monarchy of the early 1900s through two dictatorships, a republic, and a civil war, Spain has now returned to constitutional monarchy. Focusing on the key event in this chain of regimes, Raymond Carr analyzes the Civil War and relates it both to its antecedents and to the Franco regime. He acknowledges his political commitment to the Loyalist side without allowing it to become historical bias. Although analytical

rather than narrative, the work includes the results of Carr's own research on the Civil War in provincial Spanish archives.

He attributes the Nationalist victory in the Civil War both to traditional military causes and to the factionalism of the Popular Front. He points to missed opportunities caused by hesitancy or by rivalries among the political parties. The Loyalists never overcame "the weakness of the militia system: the primacy of politics over war" (p. 134). Together with regional autonomies, political rivalries prevented establishment of a unified military organization throughout the Republican zone.

In a lengthy epilogue, Carr reviews the characteristics of the Nationalist side, relating them to the nature of Franco's Spain. He cites the victory of the military dictatorship, the support of the Catholic hierarchy and the militancy of Carlist soldiers from Navarre, "the focal point of mass religious commitment" (p. 210), and Falangist control of the labor organizations as formative factors in the nationalistic, militaristic, puritanically Catholic, corporative structure of the Franco regime.

Two editorial decisions seriously detract from the book's value. Although written before Franco's death, *The Spanish Tragedy* was published in 1977; except for the addition of a postscript, the text was not altered, "in order to prove the inadequacy of historians as political prophets" (p. viii). Carr's inaccurate prophecies, despite the glib explanation, rendered an important section of the book obsolete even before publication. Revising even the last twenty pages would have greatly enhanced its value.

The second editorial decision—omission of the scholarly apparatus in the English edition, while referring the reader to a promised Spanish version—limits the value of this work to students and scholars. Footnotes are almost completely lacking, the bibliography is much too selective, and even the title and publisher of the Spanish edition are omitted.

Despite these reservations, and despite several annoyingly injudicious comparisons (e.g., contrasting the uprisings in Valencia and Málaga with the one in Ávila without mentioning the different sizes of these cities [p. 76]), this work relates the various phases of twentieth-century Spanish political life. Scholars may gain a greater appreciation of the causal relationship between political developments in the Nationalist areas of Civil War Spain and the evolution of the Franco regime. Carr's original research constitutes an important contribution to historical knowledge and points up the need for further research in local Civil War history. Finally, *The Spanish Tragedy* can serve as a

useful and interesting introduction for the casual reader or the student.

WILLIAM J. IRWIN  
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EDWARD C. HANSEN. *Rural Catalonia under the Franco Regime: The Fate of Regional Culture since the Spanish Civil War*. New York: Cambridge University Press. 1977. Pp. x, 182. \$14.95.

The aim of this anthropological study is "to trace the fate of rural Catalanian culture under the modernization program instituted by the Franco regime," and further, "to analyze the transformation of the Catalan bourgeoisie from a class that once aggressively pursued the goals of the autonomous national development to a docile dependent of an authoritarian regime." It is based on the author's extensive fieldwork in the Alto Panadés region of Catalonia, centering on the city of Villafranca, southwest of Barcelona.

The Catalans initiated the Spanish industrial revolution in the nineteenth century and led the struggle for regional autonomy against Madrid-imposed centralism. Hansen outlines the history of these aspirations but concentrates on three Catalan institutions and their origins and transformation by the Franco regime. These are the *rabassa morta* system of sharecropping, whereby tenant farmers were granted land tenure contracts based on the life of the vine; the *hereu-pubilla* marriage and inheritance system, in which property was transferred by primogeniture upon the heir's marriage and which obligated the other children to extend the family's wealth; and the cultural-recreational voluntary associations, which promoted Catalan autonomy and culture.

The author shows how both the *rabassa morta* and *hereu-pubilla* systems have gradually been disappearing under the onslaught of Madrid's national economic planning; certainly this would be expected in any nation undergoing economic transformation. More interesting is his discussion of the replacement of the voluntary associations by a bar culture; since the Franco regime outlawed the Catalan associations, businessmen have taken to the bars to transact their affairs.

The book contains a number of interesting insights into the regime's political repression; hostility to this repression was directed against local officials rather than Madrid, because local officials were the ones people came into contact with. His description of the use of political and economic power in Franco's Spain—power has been wielded by central and southern Spaniards to the detriment of Catalan independence, and these "other"

Spaniards have encouraged foreign investment and dependency on industrialized Europe—is not unique, but it is well proven. There is an appendix which gives excellent advice for foreigners attempting oral interviewing in Spain.

JOSÉ M. SÁNCHEZ  
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JØRGEN HÆSTRUP. *Secret Alliance: A Study of the Danish Resistance Movement, 1940–45*. In three volumes. Translated by ALISON BORCH-JOHANSEN. (Odense University Studies in History and Social Sciences, numbers 35, 37, and 41.) Odense: Odense University Press. 1976–77. Pp. 321; 393; 429. 80 KR each.

Since few scholarly works on the German occupation of Denmark during World War II are available in English, it is fortunate that this work has been translated. Jørgen Hastrup is the Danish historian who has done most to illuminate the occupation period, not only in these three volumes, but in a variety of other works as well. *Secret Alliance* was published in Danish between 1954 and 1959; for this English edition, however, the author has made minor alterations and included additional material based on recent research by himself and others. The work is a major contribution to the history of the European resistance movements. In particular, the work illustrates how the British Special Operations Executive (SOE) contributed to the buildup of resistance groups behind enemy lines.

Hastrup's source material is primarily some seventy archives of resistance leaders, supplemented with interviews and memoirs. In the first volume, which was Hastrup's doctoral dissertation, special attention is given to the relationship between Denmark and Great Britain during the first years of the occupation. After much difficulty the SOE succeeded in establishing a firm connection with Danish resistance groups, but the work was hampered by an acute lack of weapons and explosives. Furthermore, few Danes were attuned to sabotage and other resistance activities during the first three years of the occupation. The Germans allowed the Danish parliament and cabinet to function, and leading Danish politicians saw it as their task to get the country through the war with as little damage as possible.

In the second and third volumes, Hastrup describes the gradual strengthening of the resistance movement, which gained momentum after the Germans removed the Danish cabinet in August 1943. The tension between the "old politicians," who were leery of challenging the Germans, and

the leaders of the resistance movement, who maintained that Denmark must make a contribution to the struggle against National Socialism, is examined in detail. The problem of supplying the resistance movement with weapons, which is one of the main themes, was gradually alleviated with improved parachute dropping and reception techniques. After the invasion of Normandy and the Allied sweep through France, the British as well as the Americans were able to concentrate much more on arming Danish resistance groups, and a major underground army began to take shape. At the end of the occupation it included about 43,000 men. No major battle developed in Denmark, however, since the German troops in Denmark surrendered without fighting.

Hastrup's narrative concentrates on the deliberations of the resistance movement's leaders; this is both the work's asset and liability. On the one hand, the author has written a work which fits into the mosaic of other works on the occupation; but for the reader who is eager to learn about the Danish resistance movement, it is somewhat frustrating that there is so little attention given to the activities of the resistance fighters at the grass roots level. The most significant question about the resistance movement—what did it accomplish?—is only answered in a fragmentary manner. Hastrup mentions research by Aage Trommer which indicates that the railroad sabotage was of less significance than has generally been assumed, but there is no investigation of the impact of industrial sabotage on the German war effort. A full examination of this matter, however, might require a separate volume. Hastrup does describe in much detail how the leaders of the resistance movement decided to compromise with the "old politicians" to form a postwar government with equal representation. This description is especially valuable, because it was a major accomplishment of the Danish resistance movement that civil strife was avoided after liberation, in contrast to the cases of Belgium or Greece, for example.

It is enjoyable to read these books, since the author is a skillful writer who knows how to make the subject absorbing to the reader. His description of the motives which prompted people to participate in, or refrain from, resistance is presented with much sensitivity; and keen insight characterizes the analysis of the political factors involved in the multifarious developments. The translation from Danish is competent, except for a few minor problems; a number of typographical errors could easily have been avoided. The three volumes, which are supplied with extensive footnotes, are very detailed; for the most part this is justified, since the author is dealing with a complicated

subject. Occasionally the main themes are obscured, and English-speaking readers might argue that some of the details could have been omitted, because they are only of interest to Danes, particularly Danes who lived through the occupation. The overall impression is nevertheless one of excellence.

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ARTUR ATTMAN *et al.* *L. M. Ericsson, 100 Years*. Volume 1, *The Pioneering Years, Struggle for Concessions, Crisis, 1876-1932*; volume 2, *Rescue, Reconstruction, World-Wide Enterprise, 1932-1976*; volume 3, *Evolution of Technology, 1876-1976*. Stockholm: L. M. Ericsson. 1977. Pp. xiv, 369; vii, 388; xv, 424.

The first volume of *L. M. Ericsson, 100 Years* covers the period from 1876 to 1932. Jan Kuuse surveys the pioneering efforts, and details the growth of the Swedish telephone company from a cottage industry to a large scale enterprise by the year 1900. By the end of the First World War the Ericsson company had extended its business to many parts of Europe and the Americas. Artur Attman, the director of the company's centenary publication project, delineates the merger with the other major Swedish telephone company, Stockholm's Allmänna Telefon AB, and takes the fortunes of the company through the Kreuger crash of 1932. In volume two the period of recovery to 1940 and the following business expansion is handled by Attman and by Ulf Olsson, who concentrates mostly on the production and marketing facets to 1976. The third volume—a collaborative effort under the direction of Ericsson's chief technical officer, Christian Jacobeus—describes the evolution of the telecommunications technology involved over the hundred-year period.

These centennial volumes were set in motion by the Ericsson Board of Directors. The result is an exhaustive look at the Swedish telephone industry and its international subsidiaries, as well as a panegyric to the company.

Attman's stress on 1918 as a turning-point in Ericsson's history is well advised. The company suffered its first reverses when the Bolsheviks took over the factories and telephone network in Lenin-grad and Moscow. Ericsson shares fell fifty percent. The difficulties were compounded by the company's failure to manufacture and incorporate automatic exchanges. Ericsson was rescued by Ivar Kreuger, who, by 1930, owned a controlling number of shares. But Kreuger was also involved in an \$11 million deal with I.T.T.—an Ericsson competitor, of course.

Following Kreuger's suicide—"the shot in Paris" as it is described (2: 3)—Ericsson was salvaged by the Swedish government and by the banks, so that by 1940, with much reorganization, the company regained its stability and in the post-war period was in a strong position to help rebuild Europe. I.T.T. sold its shares in 1960, and since then the company has increased its strength from a Swedish base. At the same time, the major reasons given for the outstanding failure of Ericsson in the U.S. are poor long-range planning and badly synchronized factory processes in Buffalo, rather than the domination of the Bell system, which receives scant notice.

The glossy volumes abound in statistics and colorful charts drawn from the Ericsson archives and the Official Statistics of Sweden (full documentation is only in the Swedish edition). Business profiles spring from the pages—there are forty-four tables in volume 1 alone, which detail such facts as the drop in letters per foreign traffic telegram between 1901–05 and 1906–10 from 30 to 28 (1: 15). At the end of volume 2 we are treated to some mini-biographies, including one of the fifty-year veteran "Sharper-Kalle," a photogenic shaping operator. Indeed, this is a history written from the point of view of individuals, be they directors or machinists.

The celebratory quality of the volumes sometimes sits awkwardly with the close attention given to a myriad of facts. One learns much about the Ericsson company, its contacts abroad, business infighting, and the role of the Swedish banks, yet the final impression is one of a vast report which concludes that Ericsson is, indeed, a world-wide enterprise.

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JAN BRANKAČK and FRIDO MĚTŠK. *Geschichte der Sorben*. Volume 1, *Von den Anfängen bis 1789*. (Akademie der Wissenschaften der DDR, Schriftenreihe des Instituts für Sorbische Volksforschung.) Bautzen: VEB Domowina-Verlag. 1977. Pp. 331. 15 M.

The Slavonic tribes which in the early Middle Ages were settled, roughly speaking, between the rivers Saale and Bober have been allotted an obscure place in the subsequent history of Central Europe. Having succumbed to German invaders by the beginning of the eleventh century, they were condemned thenceforward to a condition of bondage and degeneration. Their entire history is one of decline. By the end of the eighteenth century, as we learn from this book (p. 307), the territory they

occupied had shrunk to less than half of what it was around 1500. In three hundred years the number of their villages had fallen from 1,850 to about 1,000. Nevertheless, they have managed to survive into the second half of the twentieth century. Known in English as Wends or Sorbs, they now number perhaps 70,000 and occupy parts of the *Bezirke* of Cottbus and Dresden. Their existence and rights are acknowledged in the constitution of the German Democratic Republic, and they have their own ethnological research institute at Bautzen, linked to the Academy of Sciences of the GDR. It is thanks to this institute, the *Institut za serbski ludospyt*, that we now have volume one of a new comprehensive and reasonably scholarly history of the Sorbs. The one-volume *Historija serbskeho naroda* by W. Bogusławski and M. Hórník, published in 1884, will not even bear comparison with its successor, so different are its scope and methods. Volumes two and three of the present work were published in 1975 and 1976 respectively. The fourth and final volume is expected in 1979. All four volumes will eventually be available in both German and Sorbian versions.

Volume one is divided into two main parts, the first of which is principally the work of Jan Brankačk. It covers the period from the *Völkerwanderung* to the end of the fifteenth century and deals with the establishment of feudalism in Lusatia, the process whereby the great majority of Sorbs became the bondsmen of German lords. The second part, which traces the decline of the feudal order from 1500 to 1789, is by Frido Mětšk, a scholar whose published work on this period, especially in the field of historical demography, has already put him in a position of unrivalled authority.

For the most part, the book has been compiled and condensed from a mass of work previously published in articles and monographs. The effects of compression are sometimes apparent, and they are not alleviated by the fact that references are stinted throughout. It is, for example, stated boldly (p. 216) that the Slovene grammarian Adam Bohorič took the Lower Sorbian text of the Lord's Prayer, published in his *Arcticae horulae* . . . (1584), from Albin Moller's hymnbook and catechism of 1574. Fairness would surely demand reference to the alternative explanation given by Jan Petr (*Lětopis Instituta za serbski ludospyt*, Series A, 12/1, pp. 6–7).

Whatever its shortcomings, however, this book has no rivals, and no one interested in the subject can afford to be without it.

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ANTON SCHINDLING. *Humanistische Hochschule und freie Reichsstadt: Gymnasium und Akademie in Strassburg, 1538–1621*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 77.) Wiesbaden: Franz Steiner Verlag, 1977. Pp. xiv, 441. DM 88.

The renaissance of scholarly interest in Strasbourg's Reformation will be well served by this erudite study of one of the city's most famous institutions, the Gymnasium and—after 1568—the *semiuniversitas* of Johann Sturm. In the first of the book's two rather unrelated sections may be found all the information one might seek about the school's institutional development. Anton Schindling argues that both the school's founders (Sturm, but also the city's chief reformers, Martin Bucer and Wolfgang Capito) and the city fathers consciously sought to create a community of students and scholars different from the traditional universities of late medieval Germany. Despite Sturm's attempt in 1570 to secure the school's right to self-governance, as a counter to the authority of the church, most parties were agreed that it should remain subject to the direct supervision of the civil authorities rather than becoming an independent corporation as were the other (princely) universities.

The second section too is chock-a-block with information. Here may be found Sturm's familiar ten-class organization of the beginning curriculum and the names, subjects, and lecture topics of every preceptor and professor, to the extent of the available evidence. In addition, the author has provided a thorough analysis of Sturm's humanist pedagogy and his partially successful effort to create both a *nobilitas literata* and a *nobilitas litteraria*. This attempt to found all learning upon rhetoric was another aspect of this school that was to be an alternative to the traditional university. The arts faculty was to dominate.

At base, this remains two not-very-well-integrated books, for all its value. One is an institutional study, the other an essay on a famous example of sixteenth-century education. The two do not necessarily lead to the stated (and sensible) conclusion that Strasbourg's school was a civil institution that served the purposes of a bourgeois laity. One may ignore the fact that a lack of matriculation records makes it impossible to prove how many of the sons of Strasbourg's oligarchs attended it, or to determine what they acquired in doing so. But it is difficult to avoid pursuing the fact that changes in the school's constitution proceeded from conflicts with the church, that such conflicts had a highly technical theological character, and that they enlisted the keen interest of the

civil authorities. One is left wondering if all parties may not have intended the school to produce more than a liberally educated and politically loyal elite. In the absence of a more analytical approach to the school and its curricular content (in the higher faculties in particular), one cannot know.

Nonetheless, Schindling's study is a valuable addition to the literature both on Strasbourg and on sixteenth-century education, and he should be praised, honored, and thanked for it.

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HANS-ULRICH WEHLER. *Bibliographie zur modernen deutschen Sozialgeschichte (18.–20. Jahrhundert)* and *Bibliographie zur modernen deutschen Wirtschaftsgeschichte (18.–20. Jahrhundert)*. Göttingen: Vandenhoeck and Ruprecht, 1976. Pp. x, 269; x, 242. DM 14.80 each.

Hans-Ulrich Wehler is, without question, one of the most influential, prolific, and controversial of contemporary historians. His reinterpretations of modern German history, his distinctive efforts at integrating history and the social sciences, and his discussions of historical methodology are widely known and debated. The items under review are of particular interest because they not only present well-organized and extensive selective bibliographies on German social and economic history since the eighteenth century, but because, according to the Bielefeld historian, they are essentially the personal working lists which he has compiled during the last twelve years of teaching and research. Each volume consists of approximately three thousand items organized into fifty categories, which are further grouped into general and reference works (bibliographies, periodicals, handbooks, and the like); methodological, theoretical, and interdisciplinary works; and specific topical and chronological subjects. Of these groupings, the first two are the more comprehensive. All categories include monographs, journal articles, and selections from collective works and dissertations, with older works prominent in the midst of more recent efforts.

The two volumes do have some distinct limitations. Only English, French, and German language works are cited. Eastern European and Soviet scholarship is but sparsely represented. There are no annotations. There is also the annoying, if increasingly common, omission of publishers from the citations. Despite these minor drawbacks, the two bibliographies are highly valuable tools at an unusually modest price. One can eagerly await the

companion *Arbeitsbücher* on historical theory, revolution, and imperialism which Wehler promises.

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HANS LIEBESCHÜTZ and ARNOLD PAUCKER, editors.  
*Das Judentum in der Deutschen Umwelt, 1800–1850: Studien zur Frühgeschichte der Emanzipation.* (Schriftenreihe wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 35.) Tübingen: J. C. B. Mohr. 1977. Pp. xii, 445. DM 98.

This book is the fourth and last in a series of collective volumes on modern German Jewish history sponsored by the Leo Baeck Institute. It contains nine contributions by seven authors covering a variety of subjects, both general and specific, but does not lay claim to comprehensive treatment of the theme. The most striking omission, a connected treatment of Jewish political status during the period, is perhaps justified by the extensive treatment this subject has received in the past.

Among the contributions, those of Hans Liebeschütz in intellectual history and Jacob Toury in social history are outstanding and of broad interest. Liebeschütz takes issue with the position, held especially by Israeli and younger scholars, that German Jewry's positive relation to its cultural environment was based on a conscious or unconscious reluctance to appreciate the potentially destructive elements which achieved complete ascendancy only in the Third Reich. Liebeschütz argues that German idealism in the first half of the nineteenth century offered a common ground of creativity which resulted in significant Jewish cultural contributions. Yet the most prominent figures to whom he devotes attention, Heine and Börne for example, found it necessary to make an official break with Judaism even though they continued to feel themselves "outsiders" and to be regarded as such by most Germans. Liebeschütz is especially interested in disclosing close social and intellectual connections between Germans and Jews. He devotes particular attention to William von Humboldt who—typically for his circle—favored Jewish political and social integration while anticipating a concomitant rapid demise of what he regarded as their outworn religious tradition. This denigration of Judaism, shared by nearly all German intellectuals, did not, however, prevent the rabbinical leadership, which was concerned with preserving Jewish religious ties, from being crucially influenced by German philosophy and scholarly method. Liebeschütz suggests, for the first time, a close connection between the conservative Zechariah Frankel and the

Historical School of Savigny as well as between the reformer Abraham Geiger and the more critical Tübingen School of F. C. Baur. While Liebeschütz's arguments are persuasive, his focus upon the absorption of environmental influences can give only a partial view, since positions and approaches were determined not only by the intellectual milieu but also by issues internal to Judaism, such as historical continuity and the reform of traditional institutions.

The first of Toury's two valuable contributions presents a thoroughly researched and inductively constructed history of Jewish economic and social integration. Relying upon local statistical data, both archival and printed, Toury is able to show how a Jewry, which at the beginning of the nineteenth century was for the most part economically and socially separated from its environment, underwent a process of *embourgeoisement* that resulted in a considerably greater degree of economic integration within two generations. He holds that while the process originated in the privileges which German rulers and municipalities extended to individuals, it was decisively determined by the availability of Jewish wealth and initiative at a time when the German middle class was expanding in response to new economic opportunities. A wide stratum of Jewry, at least in the cities, also sought social and cultural integration, but this proved to be problematic and hence less pervasive. Toury's more narrowly focused second piece suggests that the Revolution of 1848 represents a turning point in the inner life of German Jewry. The sense of Jewish cohesion, which most Jews still possessed in the *Vormärz*, was rapidly dissipated by the shared political hopes that arose in the atmosphere of revolution. Jewish identity, already reduced to its religious component, now suffered further erosion as politics became a substitute for religion. Toury may overstate his case when he concludes that 1848 marked "the end of ghetto existence," but he succeeds in documenting the immense extent to which it hastened a process already under way.

The remaining essays are of mixed quality. Johanna Philippson sheds considerable light on various hopes and concerns of the period through a study of Ludwig Philippson's activity as editor of the leading German Jewish newspaper, the *Allgemeine Zeitung des Judentums*. Jewish orthodoxy receives proper attention in Pinchas E. Rosenblüth's essay on Samson Raphael Hirsch. Less valuable are the rather diffuse piece by H. G. Reissner on Germans and Jews in the Age of Romanticism, and the tendentious, uninformed chapter on secularization in Jewish education contributed by Julius Carlebach. Gerhard Schulz's synthetic es-

say on the specific characteristics of German nationalism is well done, but out of place since it makes few explicit references to German Jewry.

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ERWIN GATZ, compiler. *Akten zur preussischen Kirchenpolitik in den Bistümern Gnesen-Posen, Kulm und Ermland, 1885-1914*. (Veröffentlichungen der Kommission für Zeitgeschichte. Series A: Sources, number 21.) Mainz: Matthias-Grünwald-Verlag. 1977. Pp. xcv, 283.

This volume in the series of publications by the *Institut für Zeitgeschichte* contains 143 documents from the holdings of the *Politisches Archiv des Auswärtigen Amtes* in Bonn. They begin in 1885, when the *Kulturkampf* was winding down in most of Prussia. In these several dioceses, however, the struggle continued unabated until World War I, except that, as these documents make clear, it was a matter of nationalist more than Church-state issues. In fact, this collection might more properly be titled *Polenpolitik* than *Kirchenpolitik*.

Erwin Gatz, of the Campo Santo Teutonico in Rome, where he is also director of the Roman Institute of the Görres Society, has edited and presented these documents in exemplary fashion. There is an extensive introduction, annotating the documents and providing an overview of Prussian Polish policy during these years on the basis of the German-language literature. An informative biographical sketch is provided for each personality mentioned. A short statement of the contents precedes each document. The marginalia, often so difficult to decipher and attribute, are included as well.

Most of these documents, 75 percent or more, deal with the Poznanian diocese (Poznań-Gniezno), home of the archbishop and of most of the nationally-conscious Prussian Poles; the rest deal with the West Prussian Bishopric of Kulm and the East Prussian Ermland diocese. They consist mostly of correspondence between Berlin and the provinces or Berlin and Rome. The chief item of business is the appointment of new archbishops and other high officials. Thus they cluster around the deaths of Poznanian archbishops in 1886, 1890, and 1906. They also reflect the determination of the Prussian government to have a German archbishop in Poznań, which it achieved only once, in 1886-90, with Julius Dinder. This appointment proved disappointing; Dinder was either unable or unwilling to govern his archdiocese much differently than a Pole would have done. Nonetheless,

the government pursued its goal, allowing the archbishop's chair to remain vacant for eight years after Archbishop Stablewski's death in 1906, until World War I compelled measures to reinforce the loyalty of its Polish-speaking citizens. The reopening of seminaries after the *Kulturkampf*, problems of religious instruction in the public schools, and the attitude of Church leaders to the growing national struggle in the "eastern marches" are other issues upon which these documents shed light.

One should not expect to find major surprises in this collection, primarily because the holdings of the *Politisches Archiv* have long been easily accessible to historians. But they do provide much useful information for the specialist and create or reinforce a number of impressions, most of them not very flattering to top officials of the Prussian government. Prussian ministers like Alfred von Gossler and Konrad Studt emerge here as petty, aggressive nationalists. More surprising is the impression these documents give of Cardinal Kopp of Breslau: a servile instrument of the government to which he clearly owed his high offices, his loyalty more to the state than to his fellow churchmen, especially if they were Polish. His submissions to Berlin are scarcely distinguishable from those of subaltern state officials. The government obviously sought obedient tools for top Church positions and was less interested in ability or outstanding priestly qualities. Even Germans of high ability were held back because they refused to endorse government policy toward the Poles. To their credit, most German clerics seem to have resisted the lure of higher office in return for doing this, so that the difficulty of finding "suitable" German candidates is a recurrent theme of these documents.

In sum, though it deals with a fairly narrow subject, this is a volume that all universities with graduate programs in Central European or Church history will want to have.

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RUTH LINK-SALINGER [HYMAN]. *Gustav Landauer: Philosopher of Utopia*. With a bibliography edited by ARTHUR HYMAN. Indianapolis: Hackett. 1977. Pp. x, 171. \$17.50.

Despite her evident industry in hunting down new sources and her even more obvious love for her subject matter, Ruth Link-Salinger (Hyman) has written a truly dreadful little book. She has chosen a subject who in many ways is an attractive figure,

especially to those drawn to the more benign and nonviolent strains of the anarchist and communitarian traditions. But for her, Gustav Landauer is no less than a towering political thinker and stainless moral saint, who "always and at all times knew and understood himself—as he knew and understood the world" (p. 24). The Landauer she paints is so consistently superb that she denies any significant development in his intellectual and political career. Unlike most previous biographers, she believes all his mature ideas developed by the early 1890s, when he was in his 20's, and argues that even his evidently late interest in Jewish issues was a "consistent and unbroken" element of his thought (p. 79). When it comes to evaluating his seemingly erratic and self-destructive behavior in the Munich rebellion of 1919, which has troubled earlier commentators, she blithely assures us that he was being perfectly authentic and consistent, an assurance based on the ingenious premise that "to pour out one's love and mania [*Wahn*] was a dialectical prerequisite for being one of the 'greats,' one of the heroic figures, one of the choice spirits who hang their all on eternity and eternal verities" (p. 67–68).

Inevitably, her hagiographical vision of Landauer's thought and personality leads to a bloated conception of his historical significance. Thus, for example, she sees him as "one of that generation which helped to deal the deathblow" to German state-worship (p. 58)—a characteristic whose demise, in the light of the Nazi era, she reports rather prematurely. Similarly, she argues against George Lichtheim's minimization of the libertarian impulse in German socialism, as compared to the French of the same period, by claiming that the *Berliner Jungen*, those early critics of SPD political timidity she rightly associates with Landauer, "shook the German Socialist movement to the core" from 1891 to 1896 (p. 38). This assertion, like many others in the book, is ungrounded in any serious investigation of the extent and depth of their influence. She offers no evidence that the *Jungen* had a mass following beyond their narrow intellectual circles because none can be offered. Syndicalism in France with its Proudhonist roots was a far more important phenomenon, even if, *pace* Peter Stearns, we do not grant it the popularity we once did.

Perhaps even more interesting, to stay with the *Jungen* for a moment, is their ultimate political resting place, which the author ignores. As non-democratic antiparliamentarians, they tended to gravitate to the right in the years after their expulsion from the SPD. One of their leaders, Paul Ernst, in fact ended his life a Nazi sympathizer. The ambiguity of their libertarianism, which Link-

Salinger uncritically celebrates, must be understood as extending to others associated with Landauer, in particular to his intellectual mentors, Eugen Dühring and Proudhon. Both of these figures were notorious anti-Semites, which seems never to have seriously bothered Landauer, who in fact translated Proudhon's *Jésus et les origines du christianisme* in 1913. Link-Salinger calls the translation one of Landauer's "important contributions" to the Jewish question without pausing to reflect on the anti-Semitic sentiments it espoused (p. 80). She is therefore at least consistent in finding nothing to question about his "cordial relations, within a private context" (p. 76) with the anti-Semite Hans Blüher or his "affinities in language, political style, and social composition of adherents" (p. 18) with the Action Française.

What makes Landauer so fascinating a figure, of course, was his ability to turn the often regressive tendencies of populist antimodernism in a relatively benign direction. As Eugene Lunn shows in his *Prophet of Community*, a far better book which Link-Salinger snidely deprecates throughout her text, Landauer is best understood as a leftist *völkisch* thinker who did not escape all the contradictions of the *völkisch* tradition. These contradictions were surely responsible in large measure for his ultimate political failure, which Link-Salinger never faces squarely. For her, it is enough that he be a "man of eternal paradox, of eternal mystic passion, and of eternal redemptive hope" (p. 88) and "... the bridge between a dying century and a burgeoning new one" (p. 118). Never mind that the bridge collapsed as he tried to cross it and the century "burgeoned" in an unexpected direction.

Finally, if it is not already clear from the snippets of her prose that I have quoted, Link-Salinger's mutilation of the English language surpasses even that of the period she purports to discuss. There are many choice examples, but the following will suffice: "The writer wants to call serious attention to the fact that this short sentence reflects the very reverse of the German situation in Wilhemian Germany and may well be utilized in reverse reasoning to establish the reason for the vitality of communal anarchism and revolutionary populism, to the extent to which these were still possible within the framework of a strict censorship" (p. 18). Equal atrocities are committed in her use of the footnote form, her presentation of other writers' arguments, and her frequent reliance on the "zeitgeist" as an explanatory last resort. In fact, with the sole exception of the useful bibliography of Landauer's works, whose editing she credits to Arthur Hyman, there is little in this book that justifies its publication. Serious students



of Landauer's life and work are well advised to turn elsewhere for reliable instruction.

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ANDREAS HILLGRUBER. *Deutsche Grossmacht- und Weltpolitik im 19. und 20. Jahrhundert*. Düsseldorf: Droste Verlag. 1977. Pp. 389. DM 58.

This collection of essays on Germany's foreign and military policies from Bismarck to Adenauer, by one of the foremost German diplomatic historians, is a welcome addition to a growing literature on modern German history. Some of the essays were originally presented as lectures, and all have been published before. Their style is somewhat uneven and their contents sometimes overlap. Two essays deal with German foreign and military policy in the nineteenth century, two cover the period from 1871 to 1918, and the rest deal with Germany during the twentieth century. The author intended these essays to be contributions to a future comprehensive study of Germany as a great power. The theme of most of these essays is the problem of continuity in German foreign policy, from the foundation to the breakup of the Reich, and the author's views are, if not altogether novel, always interesting and worthwhile.

Since it is impossible to deal with all twenty essays, only the highlights are touched on here. "The Problem of Continuity from Bismarck to Bethmann-Hollweg," and "Germany's Russian Policy, 1871-1918" deal with the problem of first, how to maintain and later, how to expand Germany's position as a great power. In the latter essay Andreas Hillgruber fails to deal with Bismarck's refusal to permit Russian loans to be floated in Germany in 1887/88, (the so-called *Lombardverbot*); at the same time, Hillgruber overemphasizes Bismarck's willingness to sacrifice the Austrian empire and abandon large parts of south-eastern Europe to Russia.

The essay on "Riezler's Theory of the Calculated Risk and Bethmann-Hollweg's Political Conception of the July 1914 Crisis," is an interesting attempt to relate the views of the chancellor's closest advisor to the developments of the crisis. This essay is the author's contribution to the Fischer controversy, but his conclusions, while sympathetic, do not come very close to Fischer's view of Germany's war guilt. "The 'Final Solution' and the German Empire in the East," is, according to Hillgruber, both the key to understanding and the central issue of Germany's power and *Weltpolitik* in the nineteenth and twentieth cen-

turies. In contrast to many historians, Hillgruber believes that Hitler was not a rank opportunist but that he had a clear and concise plan which included the destruction of European Jews and the conquest of Russia—the same concept that Gerhard Weinberg has described as "race and space" in his study of Hitler's foreign policy.

The essay on "General Fieldmarshal v. Rundstedt," deals admirably with the problem of an aristocratic old Prussian officer serving faithfully in Hitler's army to the very end. Little will be new to American readers in the essays "The Problem of the Second Front in Europe 1941-44," or "The Consequences of the Second World War for the Participating Powers." The final piece, "Adenauer and Stalin's Note of March 10, 1952," deals with German-Russian relations—Germany's seesaw policies between East and West during the Weimar period—and with Adenauer's total commitment to the Western alliance.

Altogether these essays are a stimulating and significant addition to our understanding of the recent past.

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IMRE GONDA. *Verfall der Kaiserreiche in Mitteleuropa: Der Zweibund in den letzten Kriegsjahren, 1916-1918*. Translated by TILDA and PAUL ALPÁRI. Budapest: Akadémiai Kiadó. 1977. Pp. 427. \$26.00.

It would be rash to suppose that the subject of the downfall of Germany and Austria-Hungary in World War I is exhausted, despite the enormous literature already extant. Whatever purposes might be served by further research, however, this book does not advance them.

Imre Gonda's research, though considerable, falls well short of the requirements of his enormous undertaking. The first major section of the work, dealing with Germany's internal political struggles and war aims, uses, at a rough estimate, one out of five of the important recent works on this subject. The coverage of the vital literature is better in the second section, on Austria-Hungary's internal history, though here too major works are omitted (e.g., those by Leo Valiani, Stephen Verosta, and D. S. Perman). The best part of the book, both in research and content, is the last, on Austro-German relations in 1916-18, which rests on fairly extensive archival research in Vienna, Budapest, and Potsdam. Again, however, many special studies relevant to this theme are passed by.

The problem is by no means merely one of what the author has consulted, but of how he uses his evidence. The work is studded with important



points asserted but never proved, arguments which cry out for supporting evidence and receive none. Complicating the matter are problems of organization. The book proceeds in highly elliptical fashion, frequently evoking a feeling of déjà vu. Individual points, instead of being organically connected, are strung together in loose chronological order, with considerable digression, overlap, and repetition. The war guilt question, for example, rehearsed at some length in the introduction, comes up again as part of the discussion of Austro-German relations in 1917 (pp. 368–71).

Most serious is the difficulty of finding an important purpose the study serves, a genuine problem it confronts and helps to solve. Virtually every major theme discussed has been treated more thoroughly and satisfactorily elsewhere. Apart from some details on Austro-German negotiations in the final section, the book tells us little that students of the period did not already know. Most of the interpretations offered—that the expansionist war aims of both powers were designed to help avoid domestic reform, that Austria-Hungary was doomed to lose its independence either in victory or defeat, that the most dangerous and disruptive nationalism in Cisleithanian Austria was German nationalism, that Austria's political structure and great-power "mission" were incompatible with the development of the nationalities toward freedom and democracy, and so on—are long familiar, even commonplace. What the author basically does is to place well-known events and interpretations into a determinist Marxist mold, invoking the class struggle and the laws of capitalist development—sometimes treated as if they were active agents rather than analytic concepts—to account for everything. One example is the Yugoslav movement, which is explained flatly as a process of national integration proceeding according to capitalist laws, with the standard accompanying circumstance of a hegemonial claim by the strongest bourgeois group (p. 230). Exactly how capitalist development created Serb and Croat nationalism, we are not told. Where the author does offer a significantly differing interpretation, the evidence tends to be lacking or to point in another direction, as, for example, in his argument that active revolutionary movements on the home front, especially among the Czechs, did much to help bring Austria down. Finally, despite Gonda's insistence that moral categories must not be used to explain law-bound historical developments, in fact the categories are frequently confounded, and the general tone of the book is relentlessly judgmental.

Admittedly no brief review can do justice to a long book, either in praise or blame. At times the

author seems to break out of his mold, proposing unexpected ideas. He suggests, for example, that the German Independent Socialists were tactically wrong in not cooperating with moderate imperialists like Bethmann-Hollweg for a compromise peace, arguing that the military dictatorship of 1917–18, though ultimately pushing Germany into revolution, also doomed the revolution to sterility and incompleteness in advance. His contention that Austria-Hungary's imperialism consisted precisely in her drive to preserve her existence is a point worth pursuing, though not, I think, within Gonda's framework of ideas. Other readers may find more such worthwhile nuggets. Few, however, will find enough to justify the struggle through this wearying book.

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HEINZ HÜRTEN and GEORG MEYER, editors. *Adjutant im preussischen Kriegsministerium, Juni 1918 bis Oktober 1919: Aufzeichnungen des Hauptmanns Gustav Böhm*. (Beiträge zur Militär- und Kriegsgeschichte, number 19.) Stuttgart: Deutsche Verlags-Anstalt. 1977. Pp. 168. DM 28.

In October 1918 Gustav Böhm, a thirty-year-old captain, was called by General Scheuch to the War Ministry in Berlin as adjutant to the minister. Unable to work with General Reinhardt, who was much more sympathetic to the Weimar Republic, he resigned in the fall of 1919. In 1945 he was executed by the Nazis. Böhm was a realistic conservative, and a keen observer of the initial months of the Weimar Republic. As early as July 1918 he was appalled by the optimism in Berlin about the war, and was far more aware than most officers of the serious political divisions at home. In October he was to argue that limited parliamentary reform might create a stronger government, and he insisted that liberal democracy did not necessarily lead to weakness, as the cases of France and Britain showed. On the other hand, he feared that Max von Baden was too weak and might not be able to hold the SPD in check. Prussian electoral reform and a tax on war profits were his suggested remedies. Böhm combined an obsessive fear of "Bolshevism"—a term that he applied very loosely—with a strong dose of anti-Semitism. The Left for him, as for so many officers, was synonymous with an ill-mannered riffraff. By November 18 he came to realize that Ebert was the best guarantee against "Bolshevism," and felt that he should be supported by the bourgeoisie. By the end of November he was convinced that the SPD had really broken with the radicals, and this was

confirmed during the Spartacus uprising in January. In May 1919 he surprisingly welcomed the Versailles Treaty on the grounds that it would strengthen German national consciousness and stop once and for all the absurd illusions of a league of nations.

The diary is not without interest, although it contains few surprises. It is painstakingly edited, and Böhm's statements are carefully checked for their accuracy. Hans Hürten and Georg Meyer prefer to present the diary to other historians with little comment rather than offer any analysis or criticism of its contents. The major impact of the diary is to give more ammunition to those historians who see men like Ebert and Noske as key figures who contained, controlled, and diverted the "revolution" of 1918/19. Their supporters will be confirmed in their opinion that they served Germany well, saving the country from chaos and even civil war. Their critics will see this as further evidence of their betrayal of socialism. As for Böhm, his diary shows that even those officers who were prepared to support the SPD were unable to accept the republic or make a real break with the traditional political values of the officer corps.

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HAGEN SCHULZE. *Otto Braun oder Preussens demokratische Sendung: Eine Biographie*. Frankfurt/Main: Propyläen. 1977. Pp. 1,094. DM 78.

Scholars will welcome this massive biography of Otto Braun, the perennial prime minister of Weimar Prussia. Except for studies by Enno Eimer and Hans-Peter Ehni, there has been surprisingly little intensive research on Prussia's role in the first republic. Moreover, for exhaustive research and comprehensive treatment, this work easily surpasses the few extant biographies by Steffen, Kuttner, and Beer, and the unpublished Stanford dissertation by Eric Kohler.

Hagen Schulze, lecturer at Kiel University, devoted five years (1971–76) to writing what is perhaps an unnecessarily long assessment of Braun's contribution to German democracy. Schulze does not, however, pretend to have written the "definitive" biography. Nevertheless, starting with a commission to research Braun's partial *Nachlass* in the archives of the *Stiftung Preussischer Kulturbesitz*, Schulze extended his explorations to virtually all relevant unpublished sources in German national and state archives (except for the DDR) and to *Nachlässe* in the *Archiv der Sozialen Demokratie* (Bonn) and the IISH (Amsterdam), and to a myriad of published sources and secondary works. The result, despite some prolix digressions, is the

most important biography of a Weimar statesman yet to appear.

A radical East Prussian in his youth, Braun is depicted in his prime as a hard-headed, domineering, managerial type. A practical politician who was preoccupied with administrative and organizational matters, he detested theorists. He pursued statesmanlike goals—support of Stresemann's foreign policy, strengthening of national unity "through and over but not *against* Prussia" (p. 317), political democratization of Prussia, defense of its territorial integrity against the efforts of Reich ministers to liquidate Prussia, and aggrandizement of the tightly articulated "democratic bulwark" of Prussia at the expense of the small North German states.

No socialist in the Marxist sense, this "red tsar of Prussia" eschewed the nationalization of the East-Elbian latifundia. After the First World War his main concern as Prussian minister of agriculture was not to expropriate the Junkers but to avert mass hunger. Socialism for Braun was really no more than collective responsibility of the community to protect every individual from material want, a goal that could be accomplished within the existing economic order.

Schulze stresses the lordly, authoritarian disposition of Braun, who, it seems, was hewn from the same hard granite as a Junker. Yet if he had been such a determined, belligerent, *Herrscher* type, would he not have shown it by identifying the DNVP with the murderers of Rathenau and by proceeding draconically against that party as Bismarck had done against the Social Democrats? Braun's refusal to take the ministry of defense after Noske's dismissal is rightly termed a "calamity," "one of the worst errors committed by German Social Democracy," because as an energetic foe of military reaction he "might have transformed the Reichswehr into a reliable instrument of power" (pp. 296–97).

An important theme of this book is that Braun's success was substantially due to the strength of his coalition governments, whose parties, including the bourgeois DVP, were usually disposed to cooperate for the common good. This refutes the widely credited myth of the paramountcy of party over national interests and suggests an early German capability for parliamentary democracy.

The subtitle, which affirms Prussia's "democratic mission," raises the question, was it universally acknowledged or simply the ascription of Schulze? Social structure and hegemonial traditions of Hohenzollern Prussia make it problematic whether a Weimar retreat could effectively discharge anything more than a Bismarckian unifying mission. To have slighted the wider sociological causes of the failure of the "democratic

mission" is one of the few weaknesses of this book.

According to Schulze a range of factors helped to undermine Braun's power base, especially Hindenburg's decree excluding Prussia from *Osthilfe* ("the most severe defeat Braun had had to face in his whole career," p. 687) and the collapse of Braun's plan for an integrated, structural *Reichsreform*. But Braun himself must bear much blame for the decline of his power. His temporizing, democratic-legalist tactic, typical of the responsible careerist Social-Democratic leaders, had at long last left him with only sterile, stagnant goals. Schulze says they hardly passed beyond maintaining himself in power. This "stabilizing" role, starkly different from Braun's "We must have the will to power!" of 1921 (p. 345), reflected the paralyzing timidity, perplexity, and petty quarreling that had seized upon his own party. Consequently, it is unrealistic to indulge the chimera, as does Schulze, that this stolid, eclectic minister, who in twelve years had not been able to democratize anything more than the top echelon of Prussian administration, in the hour of his physical and spiritual exhaustion could have staged a *Staatsstreich* against the Reich government to block a Nazi *Machtergreifung*. Schulze is on firmer ground when he argues, as did Arnold Brecht, that any defensive resort to force by SPD, ADGB, *Reichsbanner*, or Berlin police to prevent transfer of power to Hitler might, in the face of army opposition, "have led immediately to a Reichswehr or Nazi dictatorship" (pp. 754-58).

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HUGO STEHKÄMPER. *Konrad Adenauer als Katholikentagspräsident, 1922: Form und Grenze politischer Entscheidungsfreiheit im katholischen Raum*. (Adenauer-Studien, number 4.) RUDOLF MORSEY and KONRAD RIPGEN, general editors. Mainz: Matthias-Grünewald-Verlag. 1977. Pp. xvii, 124.

This book is the twenty-first volume in the monograph series put out by the prolific *Kommission für Zeitgeschichte* and the fourth volume of Adenauer Studies in that series. Like the other volumes it is the result of exhaustive research and is based upon extensive archival and printed sources. The work is a detailed description of a small but significant episode in the history of the Weimar Republic: the August 1922 Munich congress of German Catholics, or "*Katholikentag*." These annual congresses were interrupted by the war, and although there was a revival of the custom in 1921, the 1922 meeting was the first full-scale congress since 1913. The "Catholic Days" were primarily religious and social in their function, but the prewar meetings had

maintained close ties with the Center Party organization as a matter of course, and Centrist leaders were frequently featured as presidents or speakers. The participation of the Center in postwar Weimar governments, however, forfeited the allegiance of large numbers of German Catholics, and when Chancellor Joseph Wirth delivered a passionately republican speech at the 1921 meeting there were widespread complaints about the "politicization" of the congress.

In 1922 the congress was to be held in Munich, and the recent secession of the Bavarian People's Party from the Center added to the determination of the organizing committee that the session must be "nonpolitical." In fact, it turned out to be an exceedingly political antirepublican demonstration. Bavarian monarchists played a prominent role, and members of the Wittelsbach dynasty received highest honors. Non-Catholics General Ludendorff and former Bavarian premier Gustav von Kahr were conspicuously in attendance at the beer parties sponsored by the Catholic student fraternities. The highlight of the congress was a speech by Michael, Cardinal von Faulhaber, Archbishop of Munich, in which he called the 1919 revolution "perjury and high treason . . . eternally burdened and stigmatized with the mark of Cain" (p. 119).

The cardinal's sentiments were loudly applauded but did not go unchallenged, because the organizing committee had unwittingly selected a loyal republican and democrat, Konrad Adenauer, the mayor of Cologne, to serve as president of the congress. The tradition of the "Catholic Days" required that the president should not be a native of the host city or its surrounding region, but in 1922 suspicion and hostility of conservative Bavarians toward "sow-Prussians" were at their highest level, and it was difficult to find an acceptable candidate from outside the state. Hugo Stehkämper makes it clear that Adenauer was chosen, after much indecision, because he was not associated with the Center's Berlin leadership, and, because he was a representative of the occupied Rhineland and had been involved with the move to detach the Rhineland from Prussia at the end of 1918. His political preferences were evidently either not known or were disregarded. Once installed as president, Adenauer surprised and dismayed his sponsors by making pointed remarks condemning Bavarian separatism and by insisting that at least one flag of the German republic be displayed in the meeting hall along with the ubiquitous blue and white Bavarian flags. At the close of the congress he created a sensation by publicly rebuking the cardinal for his inflammatory antirepublican speeches.

The incident touched off a heated debate in the Catholic press, and both Faulhaber and Adenauer

sent reports to the Vatican justifying their stands. Adenauer's speeches at the congress and the subsequent newspaper articles and reports are analyzed at great length by the author, who provides a kind of biblical exegesis of their contents. Since most of the relevant documents are reproduced in their entirety in the back of the book, the effect on the reader is one of unnecessary repetition. The book is unquestionably interesting and provides important insight, both into Adenauer's character and into the mood of German Catholics in the early Weimar years, but the material might perhaps have been more appropriately presented in a long article rather than in an expensive 122-page book.

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JOSEF HOFMANN. *Journalist in Republik, Diktatur und Besatzungszeit: Erinnerungen, 1916-1947*. Compiled with an introduction by RUDOLF MORSEY. (Veröffentlichungen der Kommission für Zeitgeschichte, number 23.) Mainz: Matthias-Grünwald-Verlag. 1977. Pp. 236.

Students of recent German history are indebted to Rudolf Morsey and associates who established the series "*Veröffentlichungen der Kommission für Zeitgeschichte*." Many of the commission's publications bear Morsey's name as author, editor, or collaborator. In this latest work Morsey has taken the extensive manuscript of a prominent Rhenish political journalist, reduced it to order, corrected and edited the original, and made available a useful source for the recent history of the Catholic press in northwest Germany. Josef Hofmann was drafted into the army when he completed the *Abitur* and served until taken prisoner by the British. Released in 1919 he acquired the doctorate at Münster and began a staff apprenticeship on the local Center Party newspaper. In 1929 he joined the staff of the prestigious Catholic journal, the *Kölnische Volkszeitung*, and continued in that position until the National Socialists banned all confessional papers.

A gifted writer and public speaker, Hofmann occupied a position of leadership in the Center Party. He was a "Brüning man" and served in a liaison capacity between his editorial staff and the chancellor, whom he greatly admired and whose confidence he enjoyed. Although Hofmann had written and spoken in opposition to National Socialism, and had not joined the party, he was not excluded from the accredited list of journalists. He was therefore able to take an editorial position on the liberal *Kölnische Zeitung*, the only privately-owned paper with resources and prestige sufficient

to withstand pressure from the Nazi Party's newspaper trust and to continue publication until Cologne was occupied by Allied forces.

The final chapters cover the period of military government, the reconstruction of the German press, and the founding of the Christian Democratic Union. With a public reputation and an anti-Nazi record, Hofmann was appointed chief editor of the *Aachener Nachrichten*, sponsored by the British military government. With the founding of the licensed press he became a co-licensee and chief editor of the *Aachener Volkszeitung*, a position he occupied until his retirement in 1961.

In the postwar years Hofmann's political activity took precedence over his journalistic interests. It was owing to Konrad Adenauer and like-minded local leaders, such as Hofmann, that a political party was founded in West Germany that reached beyond the confessional boundaries of the old Center Party. Hofmann's memoirs give a detailed account of the meetings of local working groups that produced the name and program of the CDU. Hofmann was a co-founder of the party in Cologne-Aachen, a member of the party steering committee, and a member of the Landtag of North Rhine-Westphalia from its inception. As chairman of the Landtag committee on education and culture Hofmann played a significant role in the reorganization of public education.

Hofmann's memoirs were dictated to a secretary and transcribed. They were excessively detailed, especially for the postwar years, so that only about one-fourth, covering the years 1916-47, was considered of sufficient interest to justify publication. The remainder of the manuscript is available to students in the archive of the Commission for Contemporary History in Bonn. Hofmann's extensive collection of records and correspondence, upon which the memoirs are based, are in the state archive in Düsseldorf.

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MICHAEL STEPHEN STEINBERG. *Sabers and Brown Shirts: The German Students' Path to National Socialism, 1918-1935*. Chicago: University of Chicago Press. 1977. Pp. vi, 237. \$17.50.

By 1931 the National Socialist student organization, the Student Union, had successfully taken over the *Deutsche Studentenschaft*, the once official central organ of the local student governments, governments which had, ironically, been sponsored by the Weimar regime in order to foster democratic experience among students in the universities and other higher centers of learning. The Nazis' success among the students predated their



success in the larger society, and an organized reaction to the Student Union on the part of the fraternities, whose influence in campus politics the union was displacing, was well underway when it was abruptly cut short by Hitler's appointment as Reich chancellor on January 30, 1933. Student politics very nearly anticipated the vicissitudes of Nazi fortunes in the larger political community.

Owing to the influence of their *alte Herren* and the protection afforded, for a time, by State Secretary of the Reichschancellery Lammers, the various fraternities maintained a diminishing measure of independent life until 1935-36, when they merged into the Student Union (as did the *Burschenschaften* in 1935) or disbanded under pressure from state and party. Only the Catholic fraternity *Unitas* survived until 1938, when it was wiped out by police order.

The greater part of this work is a detailed narrative of student politics from 1918-35, but the most interesting section is the conclusion, where the author puts forth his theories as to why students at German universities and technical institutes were susceptible to the Nazi appeal so well in advance of the adult population of Germany as a whole. He sees the "romantic ideals of German nationhood rooted in a *Volk* community" as "beliefs which attempted to counter the serious class, confessional, and regional divisions within the German nation" (p. 177). These ideals took special root in German universities, and the fraternity student of the early 1920s "larded his political vocabulary with *völkish* metaphors" (p. 173). But whereas the fraternity mentality focused on an anachronistic Reich of the past, National Socialism "offered a world view which confronted the problems of modern life directly." It "confronted" these problems by creating a mass movement which attempted, with some success, to draw recruits from all classes. It promised to unite classes into one nation. National Socialism appealed specifically to students in two ways: it offered "an integrative, unifying movement," dear to the values of the mythology prevalent at the universities, but it also held out promise of ending the job crisis, produced in part by a great overproduction of graduates, which affected the students quite directly. As the author puts it, rather ungraciously, the fraternity student's "career meant more to him than his beer" (p. 171), and during this era career was most of what higher education in Germany was about. Even more vulnerable to the Nazi appeal than the member of the elite and socially exclusive fraternity, however, was the non-fraternity student whose father was likely to be a small businessman, white-collar worker, or civil servant. (A very large number of professionals in Germany were also salaried employees of the

state.) Nearly half of the students of the period were from civil service families of one sort or another, and most of the rest aspired to that status (p. 178). Such students might be antirepublican, but they were hardly antistate. The Nazis promised such students a return to prosperity, an expanded bureaucracy, and, by driving Jews from the civil service and reducing their numbers in the professions, lessened competition in the market for scarce jobs. Furthermore, the author contends, the NS-Student Union shrewdly adapted itself to the university setting, modulating some of the blatant anti-intellectualism of the Nazi Party in a successful effort to make National Socialism more palatable to the students. At the same time the Student Union played up to student concerns with its image of a future *völkish* community in which there would obviously be careers for politically loyal Aryans. Short-term advantage, of course, turned out to be long-run disaster.

This work is based on extensive research in primary sources, which include, among others, the records of the fraternities, the *Deutsche Studentenschaft*, and the NS-Student Union (collected at the University of Würzburg), records from the *Bundesarchiv*, and documents from the Prussian Ministry of Culture and Ministry of the Interior lodged in the German Democratic Republic. The author also read many German dissertations relevant to his subject.

If the book has a fault it is that its conclusions are more enlightening and stimulating than its narrative, and the conclusions owe more to the author's informed thought and speculation than to the narrative which proceeds them.

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ROBERT G. L. WAITE. *The Psychopathic God: Adolf Hitler*. New York: Basic Books. 1977. Pp. xx, 482. \$13.50.

For the historian of this century, the figure of Adolf Hitler looms both very large and very strange. He occupies a large place because of the enormous effects his policies and actions had for the whole world; he seems strange because of the extraordinary choices he made, choices which both repel and fascinate, which invite scrutiny even as they defy complete understanding. The literature about Hitler is immense, and it is only appropriate that the psychohistorians should have their turn.

Robert G. L. Waite has devoted many years to the study of Hitler from the perspectives of psychohistory, and his findings will attract considerable attention. After presenting his view of the problem of studying Hitler, the author first describes Hitler



as a person, then reviews the origins and nature of his ideas, and in the third and longest chapter of the book entitled "The Child As Father to the Man" analyzes Hitler's youth, personality development, and sex life. Stepping into the broader arena of German history, Waite then looks at those aspects of German development which made it plausible, though not inevitable, for a person like Hitler to become chancellor, and finally looks at the implications of Hitler's personality for the history of the Third Reich under the title "From Private Neurosis to Public Policy."

Waite concludes that Hitler was a "borderline personality," a type of person who, though mentally ill, can function very effectively in some areas. The term borderline here refers to the border between neurosis and psychosis. The characteristics of such individuals appear to Waite to fit Hitler (pp. 356-59), but as the author explicitly admits, such a diagnosis cannot explain the entire personality and life history and all the decisions of a political leader. The details must be illuminated by psychohistory, and it is this that Waite sets out to do.

In his search for evidence, Waite has properly rejected the fake (as opposed to the authentic) Eva Braun diary; he demonstrates that the "memoirs" of Josef Greiner were fabricated by the latter, and he correctly deprecates many of the dubious tales of Werner Maser, Rudolph Binion, and Robert Payne. Where he deals with what one may be allowed to call more traditional problems, such as the origins and development of Hitler's ideas or the characteristics of German history that opened the way for the Fuehrer, Waite does very well. There are a few slips, but on the whole Waite has summarized the evidence on Hitler and interpreted the relevant strands of late nineteenth- and early twentieth-century German history very convincingly.

Chapters one, two, and four, point the way, nevertheless, to a fundamental problem in the other chapters: the lack of evidence on Hitler's personal life. Waite has not fully confronted the problem inherent in dealing with an early childhood about which almost nothing is known. From the personality characteristics of the mature Hitler he reasons backward, very frequently on the basis of analogies to others, in search of the formative experiences of Hitler's early childhood, which then become the explanation for his later conduct. Essentially the reasoning is circular. Where there is evidence, or appears to be evidence, the enthusiasm of the author often seems to run ahead of reality. Whatever caution he exhibits when the evidence is first cited, its assured authenticity and significance constantly grow thereafter.

Waite places great emphasis on four interrelated

aspects of Hitler's personality development: the supposedly missing testicle, the relationship with his mother, the supposed real or imagined viewing of intercourse between his parents, and the impact of his mother's death from cancer while in the care of a Jewish physician. At each of these points the evidence is even shakier than the author allows. On the first point he asserts, "Two conclusions thus seem incontrovertible: the body on which the Soviet pathologists performed the autopsy was that of Hitler; and in that body the left testicle was missing" (p. 151). Dental evidence was used to identify the charred corpse as that of Hitler. While I fully accept the identification, dental evidence cannot be adduced as proof of the missing testicle! The author relies on the reputations of the Red Army pathologists—and he may well be right to do so—but then he argues (quite convincingly in my opinion) that, contrary to the assertion in the same autopsy, Hitler was shot and not just poisoned. Political motives can be adduced to explain the pathologists' insistence that poison alone was the cause of death, but no such motive could have caused them to report that a testicle was missing. There is also the subsequent statement from Hitler's company commander, but perhaps that ought to be met with a little skepticism. In any case, "incontrovertible" is a bit strong.

If, however, one does accept the finding that Hitler was indeed congenitally monorchid, and if this had subsequent repercussions, can these repercussions be discussed only in terms of general allusions to other people, without specifying time, place, and individual? Ought not the psychohistorian, as contrasted with the psychiatrist, look further and perhaps discuss specific cases? For example, before a psychohistorical biographer of William II goes too far in explaining all manner of things in terms of that emperor's withered left arm, should he not confront the life of another important figure of this century similarly afflicted, Lord Halifax?

Because the discussion of Hitler's relationship with his mother is necessarily heavily speculative, what evidence there is should be treated with great care. In the discussion of Hitler's 1923 poem about motherhood (pp. 141-43), Waite reverses the sequence of thought in the text; he has Hitler walking his dying mother on her last mile, smiling, when the poem clearly states that you should be happy to take your mother walking in her old age precisely because before long you will be crying when you accompany her on her "last mile." Perhaps the word *Lust* is not as surprising in the actual context of the poem as Waite claims, especially when one allows for a point he ignores—the Fuehrer was trying to find words which rhyme.

The "primal scene trauma" (pp. 162-64) is dis-

cussed at great length, and its impact, along with the effects of monorchism, is perceived by Waite as critical. The basic evidence, however, is shaky, to say the least. The passage in *Mein Kampf* which the author relies on may or may not be autobiographical. Waite assumes that it is, but his proof is a passage saying that Hitler had seen such things hundreds of times—a passage that occurs several pages earlier and covers a large number of other statements that are omitted as nonautobiographical. The text in *Mein Kampf* refers to quarrels within a family which the “*Form roher Ausschreitungen des Vaters gegen die Mutter annimmt, zu Misshandlungen in betrunkenem Zustande führt.*” Waite translates this as “brutal attacks on the part of the father towards the mother or assaults due to drunkenness.” Without arguing the translation, is one really obligated to deduce that at age three Hitler saw or imagined he saw that “his inebriated father attacked his mother and did something terrible and strange to her. And, most awful of all, she seemed to enjoy it”? (p. 162). The enjoyment is Waite’s interpolation; drunkenness as a characteristic of Hitler’s father has been explicitly rejected by Waite himself, and the assumption that sexual intercourse is meant is at least as dubious as the autobiographical ascription. The German text suggests wife-beating and the reference to scenes that would make a grown-up shudder may thus mean something quite different from what Waite asserts. When in this account Hitler refers to the son repeating the behavior pattern of the father, he refers to him as beating (“*prügeln*”) the mother; Waite cites this passage (p. 169) but without indicating that this is Hitler’s way of briefly describing a repetition of what Waite believes was a primal scene.

The connection between Hitler’s anti-Semitism and the death of his mother is not exaggerated by Waite as Binion has done, but the whole subject is still very shadowy. The Jewish doctor was highly regarded by Hitler, profusely thanked, and treated well by the National Socialists. For these points we have real evidence, and Waite acknowledges them. All the negative displacement and unconscious fear about which Waite also writes (pp. 188–90) is speculation. Similarly, the discussion of Hitler’s sex life is most sensible on points where there is substantial evidence, that is, that the subject was much on his mind and that the age difference was generally the same as that between his parents. All else is based on the assumption that speculations are facts.

When the author turns to the significance of Hitler’s personality as a clue to public policy, he makes very sensible observations regarding the absence of sudden changes in Hitler’s behavior, and the centrality of anti-Semitism in policy and ideol-

ogy. When one comes to the policies themselves, however, the insights that the psychohistorical approach might have provided turn out to be mirages. We are given various reasons why Hitler may have ordered that the Jews should be killed by gassing (p. 371), but Hitler ordered them killed without specifying how—the first several hundred thousand were killed by shooting—and the earliest mass gassings were performed on “Aryans” in the euthanasia program. There are other extraordinary errors in this regard. The order to kill the Jews in Russia as the German army advanced was given to Heinrich Himmler in March 1941, and the removal of Jews from the rest of German-controlled Europe was ordered in July of the same year when Hitler thought the war against Russia had been won. Waite combines these two decisions into one and shifts it to the Wannsee conference held on January 20, 1942 (but originally scheduled for December 9, 1941). The author then uses the incorrect date to prove that Hitler made this decision as part of his recognition that victory in the eastern campaign was eluding him! If the findings of psychohistorical analysis are congruent with such chronological and causal confusion, is it a safe guide? It has misled Waite on the Jewish question, which he regards as central, and I would add that it has also misled him on the whole matter of the invasion of the Soviet Union. The massive study of Andreas Hillgruber on Hitler’s strategy has shown beyond any doubt that Hitler shared with his generals the belief that the Soviet Union was not a serious military enemy, and all of Waite’s speculations about Hitler’s unconscious search for failure must be discarded. The choices in the Mediterranean never interested a Fuehrer who always insisted that that area was the proper *Lebensraum* for Italy, while Germany’s lay in the east.

When using a new and complex procedure, one should exercise extreme care with the available evidence; there are too many places where this reviewer cannot share the author’s reading. A phrase in Hitler’s November 8, 1940 speech is considered significant enough to quote twice: “I am totally convinced that this struggle does not differ one hair’s breadth from the battle which I once fought out within myself” (pp. 386 and 387). What did Hitler say? On the anniversary of the Bierhall Putsch he spoke at great length about the struggle for power inside Germany. Then he turned to foreign policy, described as “*Kampf nach aussen*,” his struggle with foreign powers. After asserting that the war had to be fought, Hitler continued: “*Ich bin fest überzeugt, dass dieser Kampf um kein Haar anders ausgehen wird, als der Kampf, den ich einst im Innern ausfocht.*” Surely what Hitler is saying here is that this war—the military one against the foreign

enemies of Germany—will end exactly like the prior political one against his domestic foes, and a fair translation would be: “I am absolutely convinced that this struggle will end not a hair’s breadth differently from the one I once waged at home.” In another case, a passage from Hitler’s second book is quoted once from my edition (p. 388, n. 107) and once (p. 383, n. 90) from a somewhat dubious edition, without any indication that the same text is involved. As for the summary of Hitler’s euthanasia order (p. 392), the phraseology used is simply not to be found in the original text published many years ago.

In view of the author’s enormous work on a very difficult subject it is unfortunate that the foregoing criticisms, which could be extended, have to be made. Waite has infused his book with so many shrewd observations, so many helpful suggestions, and such good-humored skepticism that criticism may appear captious. But when a new approach is taken, it must be done with care and its explicatory value ought to be demonstrated. Neither of these standards is satisfactorily met in Waite’s book. The very great strengths of the book are precisely in the more traditional portions, and the weaknesses in the least traditional. The psycho-historical approach may well have new things, and important new things, to tell us about Hitler. Certainly Waite’s book will suggest to all readers new ways to think about the Fuehrer and new questions to ask of the available evidence. The psycho-historical approach also offers an opportunity to expand the body of evidence itself by drawing attention to statements, actions, and behavior not previously perceived as useful evidence for the historian. But the proof that this approach will really tell us more about the “Psychopathic God” has yet to be provided.

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ALAN D. BEYERCHEN. *Scientists under Hitler: Politics and the Physics Community in the Third Reich*. New Haven: Yale University Press. 1977. Pp. xii, 287. \$18.50.

Alan D. Beyerchen’s *Scientists under Hitler: Politics and the Physics Community in the Third Reich* is a very good book indeed. Using as his focus the interaction between the Nazis and the German physics community between 1933 and 1945, Beyerchen weaves a tapestry of important individual themes into a synthesis that transcends the ordinary monograph.

Beginning with a brief description of German academia, Beyerchen takes the reader through the dismal impact of Nazi dismissal policy on the

physics community of both the university and the research institute, using Göttingen as a focus. The response of the physics community—emigration, resistance (rare), accommodation (usual)—is well handled, with the issues and personalities involved blended in an effective manner. Following the *Machtergreifung*, struggles over political direction and interference, with the physics community on the one side and both party and state on the other, continued. The development of Aryan physics in the persons of Philipp Lenard and Johannes Stark, their efforts to aryanize physics, and their ultimate failure in the war years completes the narrative.

Throughout the study Beyerchen uses a wealth of primary material garnered from a variety of archives and combines that material with interviews, private papers, dissertations, manuscripts, books, pamphlets, journals, and articles. His research is a model in its blend of new material with published sources.

His work also is to be praised for having escaped the potential narrowness of the monograph. He achieves this first by focusing on a topic which is transdisciplinary (physics and politics). Furthermore, he consistently weaves in themes that transcend any narrow focus, such as his skillful treatment of the attempts at nonpolitical accommodation (called self-alignment) practiced by so many of the physicists in order to escape politicization or worse, an almost classic case of the nonpolitical adjusting to the politically active. Another example is his treatment of disagreements within the discipline of physics (such as the clash between theoreticians and experimenters) and the way in which they affected and were affected by the contemporary milieu, including political ideology. While focusing on the physics community and key representative figures therein, Beyerchen simultaneously focuses on questions that transcend the German physics community and the Nazis, reaching much broader issues such as academic life, research, and the role of scientists outside the laboratory.

If there is any weakness of the work, it is that there is too much detail on the Aryan physicists. The space allotted to the personal, academic, and political causes of their position could easily have been pared, as could the account of their political campaign at aryanization.

There are a few minor quarrels with sources. For example, Werner Heisenberg’s *Physics and Beyond* and *Across the Frontiers* are not included in the bibliography. Overall, however, that research tool is excellent.

In conclusion, the book seemingly sets itself a narrow problem. What happened to the physics community when the Nazis came to power? How did it survive to 1945? Those questions are an-

swered by Beyerchen. But much more is provided to the reader as the author uses those questions as a vehicle to study the fate of science and apolitical scientists in a politicized society, and to illuminate topics often neglected by historians.

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MARLIS G. STEINERT. *Hitler's War and the Germans: Public Mood and Attitude during the Second World War*. Edited and translated by THOMAS E. J. DE WITT. Athens: Ohio University Press. 1977. Pp. x, 387. \$20.00.

Can historians really determine the nature and role of public opinion in totalitarian societies? Marlis G. Steinert attempts to do so with surprising success in this chronologically-organized study of the German public's opinion about World War II (*Publikumsmeinung* rather than *öffentliche Meinung*). The key sources for analyzing "a subject still largely unknown" (p. 18) are the reports of Otto Ohlendorf's *Sicherheitsdienst*, and these "reports from the nation" (*Meldungen aus dem Reich*) are quoted generously in support of the narrative. After defining the topic and sketching the prewar mood all too briefly, the author focuses on answering three interrelated questions about the public mood and attitudes in wartime Germany: "1. to what extent were these opinions differentiated, independent, and genuine, and to what extent were they manipulated or controlled; 2. which factors influenced them; and 3. how far did their influence reach?" (p. 4).

As part of her general thesis that propaganda and public opinion influence and limit one another in a totalitarian society, Steinert concludes "that the Third Reich was only rarely supported by a unanimous public vote; opinions were far less uniform, Germans did not favor aggression nor were they enthusiastic about war. They were more likely resigned, fearful of war, and yearning for peace" (p. 7). Her narrative stresses popular anxiety about the duration of the war and the ups and downs of public mood which followed the course of the military conflict on the various fronts, as well as the grumbling caused by food shortages, the inequality of sacrifices, and the behavior of party officials (always below Hitler). The role of the churches as "the focal point for numerous criticisms of the Third Reich" (p. 330) remained ambiguous throughout the war, even though church opposition apparently forced the regime to alter the euthanasia program in 1941 and thereby may have contributed to the subsequent decision to carry out the "final solution" of the Jewish question in strict secrecy on the fringes of the Nazi

empire. In this regard, Steinert tends to support the view that Germans knew little of the regime's mass murders and were, in any case, too "engrossed in daily cares and mounting fear for their lives" (p. 335).

Some of the official police reports on the changing public mood were remarkably candid. The Stuttgart security police reported in July 1941, as the German army rolled into the interior of Russia, that "citizens are depressed, embittered, and angered by the severity of the Eastern struggles . . ." (p. 120). In 1943 the police recorded grumbling about Goebbels' policy of total war, on which some Germans blamed the increased Allied bombing activity, as illustrated by the versified black humor circulating in the Ruhr in the spring of 1943: "Dearest Tommies, do fly on/We're all miners put upon./To Berlin would be our guess,/They're the ones who shouted 'yes'" (p. 202). Given the frankness of such reports, it is obvious why Goebbels and Bormann moved to restrict their circulation as the war turned sour. One of the few weaknesses of this study is the sketchy treatment of the development of these "internal newsletters." For example, how was information gathered, how reliable were the sources of information, how was it evaluated, how many officials had access to this information, did Hitler see these "reports from the nation?" Such questions might have been confronted more directly, because the strength of this study depends so heavily on this crucial documentation. In addition, the strict chronological approach reduces the impact of valuable new information on important topics such as the attitudes of German women and workers during the war. Finally, one still wonders about the potential influence of the public's opinions on such a repressive regime. Could an accurate reading of the public mood really have changed the fundamental nature of the Nazi regime? At most, it seems, perceived public dissatisfaction could only force such a regime to tighten secrecy and be more circumspect in pursuing chosen policies.

In spite of such limitations, Steinert presents a believable and well-documented picture of the public's opinions, especially toward the war, in Nazi Germany. Hopefully, others will follow her lead in attempting similar analyses of the prewar period and selected nonmilitary topics such as the regime's social and economic policies. This English edition, carefully edited and translated by Thomas de Witt, leaves most of the original 1970 German edition intact. Unfortunately, the American publisher, deserving of our praise for providing an excellent translation of such an important work, has placed the extensive and crucial notes at the end of the chapters, thereby contributing to



acute index-finger fatigue in any reader who attempts to follow the documentation provided in more than sixteen hundred footnotes.

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DAVID IRVING. *The Trail of the Fox*. New York: Thomas Congdon Books; distributed by E. P. Dutton. 1977. Pp. 496. \$15.00.

This life of Erwin Rommel will appear to those familiar with earlier writings of David Irving to follow much the same pattern. It is another example of extraordinary enterprise and ingenuity in ferreting out material others have overlooked or have resigned themselves to do without. His success here is as dazzling as in *Hitler's War*. There is also much brilliant writing; his works, which once left something to be wished for stylistically, have gained steadily in sparkle and polish.

Those who see Rommel as a late-coming but inspiring figure in the German resistance to Hitler will perceive a repetition of a perverse humor that delights in taking issue with widely accepted popular and professional assumptions on sensitive issues. In *Hitler's War* Irving insisted that the Nazi Fuehrer never ordered the liquidation of Europe's Jewry and was not even aware of it until late in the war. In dealing with Rommel, the author maintains that he was never truly associated with the plot of July 1944.

The treatment of Rommel as military commander defies easy labels. Irving does something of a balancing act between champion and detractor. At certain points there are seeming contradictions. Thus Rommel is portrayed both as a defeatist and as an indomitable striver against insuperable odds. Like many earlier critics of the formidable Swabian, Irving notes serious failings as strategist and logician. Yet, in the end, he is given a unique accolade—that of a twentieth-century Hannibal. Since no other captain of our age, except perhaps in his own mind, has ever been elevated to such heights, comparison to the peerless Carthaginian would imply marching by himself in the first rank of the soldiers of this century.

Rommel the man emerges from Irving's study a wholly believable, intensely human, figure. He could be vain, overbearing, obstinate, and occasionally petty. His fundamental inclination was a robust optimism, without which his more startling successes would scarcely have been thinkable. As the tide of victory ebbed, he became prey to intermittent pessimism, which, Irving would have it, amounted to defeatism. Not many instances are alleged in which this could have affected his generalship.

On the positive side were Rommel's vaunted chivalry, never-failing bravery, and personal magnetism. In these respects Irving rates him well above his very conscious rival, Bernard Montgomery. In several other ways, however, the two are judged to have had common traits, being characterized as high-handed, arrogant, professional soldiers virtually devoid of intellectual qualities, and having a great flair for public relations. As a soldier, Rommel receives much higher marks for tactical insight, battlefield courage, and talent for improvisation.

The ability to include Ultra in their considerations is a great advantage to historians currently writing about the soldiers of World War II. The revelations of the last three years place much stress on how the ability to read high-level German wireless communications aided the British in a war of such intense movement as that in Africa. With Ultra coming into fullest flower almost coincidentally with Montgomery's assumption of command in August 1942, that devotee of the "set battle" could station his chessmen in such a way as to anticipate Rommel's every move. Ultra undoubtedly was the main ingredient in such British victories as Alam el Halfa, El Alamein, and Medinene.

Rommel's role in the July plot has long been the subject of vigorous and, at times, bitter debate. Irving's vehement argument against his active participation is bound to bring comfort to unreconstructed Nazis but cannot for that reason fail to receive full consideration. There can be no doubt that the champions of the military conspiracy have highlighted Rommel's role too much. It is unfortunate that General Speidel, whose vital testimony Irving dismisses as self-serving and mendacious, is prevented by a stroke from responding to these charges.

An effective device employed by Irving as a means to focus attention on key issues is interspersing his account with discussions of new evidence on challenging points. It emphasizes once more the richness of the sources—particularly correspondence, diaries, and personal testimony of survivors of the period—on which the study is based. Despite the controversial character of some of Irving's positions, which invites other interpretations, his work thus gains a certain definitive character. Aspiring biographers who are less well equipped with personally-discovered material will perforce hesitate to follow *The Trail of the Fox*.

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KONRAD LÖW. *Die Grundrechte: Verständnis und Wirklichkeit in beiden Teilen Deutschlands*. Munich: Verlag Dokumentation. 1977. Pp. 419. DM 18.80.



Hugo Preuss' appeal to the Weimar Constituent Assembly of 1919 that the government, about to be created, must be "blood of its blood and flesh of its flesh" was, judging from the West German record, heeded much more closely in 1949 than it had been in 1919. If the West German federal constitution (Basic Law) of 1949 has been a success, the more recent blows of terrorism and emergency legislation notwithstanding, then the success is due not only to the lessons learned from the mistakes of Weimar, but the pre-eminent role assigned to basic rights as well. These rights, from the beginning viewed as much more than mere recommendations, as under Weimar, have been protected and interpreted by the Karlsruhe constitutional court. The sheer volume of the cases reviewed over the past twenty-five years—33,323 in all—suggest the importance of this institution, for which there was no precedent in the Weimar Republic.

Konrad Löw's *Die Grundrechte: Verständnis und Wirklichkeit in beiden Teilen Deutschlands* looks at basic rights in both German states, the Federal Republic of Germany (BRD) and the German Democratic Republic (DDR), and it does so not only from the standpoint of the constitutional lawyer but also from that of the social historian and interpreter of ideology. Essentially conservative, the interpretation is nevertheless very well balanced and fair, touching upon a wide variety of issues in the relationship between individuals and the state and individuals themselves in both German societies. In his comparative analysis Löw juxtaposes ideological premise and constitutional promise with individual and collective existence in either half of Germany. He considers the principal distinction to lie in the existence of an independent judiciary in one part of Germany and its absence in the other. As an example, Löw cites the extensive constitutional guarantees on behalf of private property in the East German constitution of 1949, whose effectiveness until 1968 also coincided with the near-total expropriation of private farmland and privately-owned crafts and enterprises in the German Democratic Republic. There is, however, no explanation given as to why the East German constitution of 1949 was deliberately couched in Western liberal-democratic terms, when constitutions elsewhere in Eastern Europe were well on their way toward replacement by more Stalinist models.

In comparing the constitutional promise and everyday reality in West Germany, Löw does not stop with the socially tranquil and politically stable 1950s, an era which some West German publications have already begun to characterize as "distant" and "remote." Löw looks at the more heated 1960s and the sometimes explosive 1970s, which brought much more violent social chal-

lenges than the generation of the Adenauer era ever cared to witness, much less experience. In examining these social challenges and the attempts to answer and solve them on the basis of the existing social-political order under the 1949 Basic Law, Löw appears to be addressing himself primarily to West German youth and especially its academic sector. Part of the purpose of *Die Grundrechte: Verständnis und Wirklichkeit in beiden Teilen Deutschlands* is to highlight the merits of the 1949 Basic Law, and especially its bill of rights, by comparing it with the failure of German civil liberties under Weimar, the extinction of German civil liberties under Hitler, and the subsuming of German civil liberties under the overriding collective thrust of East German ideology and practice. The relevant quotations from the Scholls' courageous appeal against Hitler and Ulbricht's justification of the Berlin Wall are well employed in this endeavor.

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HONORÉ M. CATUDAL, JR. *The Diplomacy of the Quadripartite Agreement on Berlin: A New Era in East-West Politics*. Foreword by KENNETH RUSH. (Political Studies, number 12.) Berlin: Berlin Verlag, 1978. Pp. 335. \$12.90.

The Berlin Accord of 1971/72 stands today as the cornerstone of a series of historical agreements, including the Moscow and Warsaw Treaties (1970), the East-West German Basic Treaty (1972) and the Helsinki European Security Conference (1975), which together constitute what may be regarded as the post-World War II European peace settlements ending the long period of postwar ideological squabbling over territory and political recognition in Central Europe. Honoré M. Catudal's book is the first major study of the evolution of the first and key part of that historic accord, the Quadripartite Agreement of September 3, 1971. Based on a thorough review of major U.S. and German press reports and published documents as well as interviews with a number of American officials, the book presents a systematic and detailed reconstruction of the negotiations beginning in 1969, when brand new governments in Bonn and Washington combined with renewed Chinese pressures on the Soviets to create a favorable negotiation climate. Catudal's analysis moves simultaneously along three tracks: the actual Four-Power negotiations in Berlin, West Germany's own Eastern initiatives, and the coordination between Washington and Bonn. The text and a brief critical analysis of the key documents round

out the book and provide the general reader with a handy reference to the agreement.

As the official record is still classified, it is surely inevitable that the book brings no great new insights. A close reading of the German and American press would yield materially the same story. Catudal interviewed widely and asked the U.S. negotiator, Ambassador Kenneth Rush, to comment on an early draft of the book, but neither Rush nor the others interviewed apparently revealed significant classified information. The main effect of the interviews thus appears to be the book's strong identification with the American position. The revelation of one "closely guarded secret," the Allied promise to restrict the activities of the rightist National Democratic Party in West Berlin, is a bit overdramatized, given the well-known ban of NPD national congresses from Berlin in 1969 and 1975 and other restrictions.

One curiosity is the lengthy first chapter on the 1961 Berlin Wall Crisis. This part seems curiously outdated and isolated from the rest of the book. The account follows in the main Jean Edward Smith's *Defense of Berlin* (1963), a 1967 manuscript of Berlin Desk Officer John C. Ausland, Eleanor Dulles' books and a number of 1966/67 interviews rather than such recent critical and scholarly works as Jack M. Schick, *The Berlin Crisis* (1971), Walter Stützle, *Kennedy und Adenauer in der Berlin-Krise* (1973; not cited at all), and Hans Herzfeld, *Berlin in der Weltpolitik* (1973). More importantly, the chapter is not linked in any way to the story of the Four-Power Accord. This is particularly curious since the first crucial impulses toward a Berlin settlement—"interim agreement" or "modus vivendi," as it was called then—date back to the Kennedy years: the Rusk-Gromyko/Thompson-Gromyko Talks in Moscow (1961), Kennedy's crucial Berlin Free University speech in June 1963 (which was cited often by Brandt and his associates in their efforts toward technical agreements in Berlin), and perhaps most important of all, JFK's cool reply of August 19, 1961 to Brandt's impetuous letter immediately after the building of the wall, in which the president apparently admonished Brandt that more than confrontation politics was needed in Berlin.

Still, for the negotiations from 1969 to 1971, Catudal's diligent work of historical reconstruction is likely to remain the standard source for some time to come, superseding such early policy studies as Dennis L. Bark's *Agreement on Berlin* (1974). May the second volume, in which the author will "attempt to evaluate the Quadripartite Agreement after five years of implementation" be equally successful!

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JOHN P. SPIELMAN. *Leopold I of Austria*. New Brunswick, N.J.: Rutgers University Press. 1977. Pp. 240. \$14.95.

No reign was more important to the destiny of the Habsburg monarchy than that of Leopold I. As Holy Roman Emperor from 1658 to 1705, Leopold repelled the last Turkish invasion of Central Europe and subsequently reconquered Hungary for his dynasty. As a lifelong adversary of Louis XIV, he also proved to be a key figure in virtually every European coalition against the Sun King. If Leopold never won the acclaim of his contemporaries or of modern historians, it has been because both his heavy reliance on confessional considerations and his overall indecisiveness in executing domestic and foreign policy compared so unfavorably with the bold pragmatism of so many of his fellow monarchs.

In the first English-language biography of Leopold I, John P. Spielman presents us with a lucid and comprehensive chronology of this eventful reign and also offers a balanced evaluation of the personality of the Habsburg emperor. To a certain extent Leopold is portrayed as a typical Habsburg; he tended to avoid challenging the privileged position of the Church and nobility within the Habsburg dominions because he looked on them more as indispensable supports to what authority he had than as obstacles to any strengthening of his sovereignty. In addition, however, the author argues persuasively that Leopold was motivated throughout his life by the strong religious outlook bred into him during his childhood preparation for the priesthood. When, as emperor, he allowed domestic and foreign affairs to drift and permitted others to take the initiative, it was because he saw events "not as the working of blind chance but rather as the unfolding of God's design" (p. 200) to which he could only make minor and imperfect adjustments. When he fought Louis XIV, he did so largely out of moral outrage at Louis' perfidy in allying with the infidel Turks or in subverting divinely sanctioned treaties and laws of succession. The author frequently criticizes Leopold's indecisiveness and extreme confessionalism, for example his failure to act swiftly in allying with the Protestant maritime powers against France or in composing religious differences with his Hungarian subjects. Nevertheless, his portrayal remains essentially sympathetic. Moreover, it is difficult to dispute his assessment that "what the Austrian empire eventually became has much to do with the gentle policy of Leopold I" (p. 27).

Given the monumental contributions of Arneth, Redlich, and Braubach it is not surprising that Spielman's research breaks no new ground. In

fact, with the exception of some insightful looks into Leopold's musical compositions, the author cites virtually no archival material and relies instead on the major published primary and secondary sources. Footnotes are scanty, averaging fewer than one per page, and many of these only dispense general bibliographical information. The book contains a number of factual errors, mostly concerning minor details. In his penultimate chapter, however, the author commits two mistakes worth pointing out. Entitled "The Opposition Takes Over," the chapter operates on the assumption that, after the great purge of Leopold's inner circle in June 1703, the reform faction headed by the emperor's son Joseph gained permanent control of the government. In fact both Braubach and existing archival material make it abundantly clear that the old guard soon regained the upper hand and largely excluded the Josephine faction from power. In his discussion of the *pactum mutuae successionis* Spielman also describes incorrectly Leopold's provision for a female succession, making it essentially identical with the terms of the Pragmatic Sanction of Charles VI and thereby blurring the tremendous significance of Charles' will.

Notwithstanding these shortcomings the author does present us with a well-written, well-organized history of a critical period in Habsburg history and a useful profile of his subject. The volume itself is attractively presented, with illustrations, helpful (though somewhat inaccurate) maps, and a detailed genealogy. Moreover, as the first and only English-language biography of Leopold I, Spielman's work should prove indispensable to students and useful to specialists in Austrian history. As such it would be a valuable addition to any college or professional library.

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ALEXANDER GERSCHENKRON. *An Economic Spurt that Failed: Four Lectures in Austrian History*. Princeton: Princeton University Press. 1977. Pp. x, 171. \$12.50.

Like almost everything written by Alexander Gerschenkron, this book is insightful and a pleasure to read. The book originated in the Janeway Lectures at Princeton in 1972, which were then partially rewritten on the basis of further research. The work reveals in full spectrum the advantages and disadvantages of a lecture style. On the one hand, the reader is witness to a striking display of the wide range of Gerschenkron's erudition, as well as his reflections on a number of questions in European history. On the other hand, this welcome spontaneous quality of a lecture also permits both

a range of speculation without supporting data and some personal comments, at times quite acerbic toward his colleagues, which often seem overreaching and unwarranted.

The major focus of the work is an examination of the economic program of Ernest von Koerber, Austrian prime minister from 1900 to 1904. Upon his accession to power von Koerber introduced a broad-ranging public works program, including vast railroad and canal development schemes. At a time of paralyzing strife between the national groups in the Austrian parliament, the introduction and passage of the program restored a brief period of unity and tranquility to Austrian political life. Gerschenkron argues that, in addition, the program could have taken Austria into the "great spurt," leading to economic growth on a large scale. However, according to Gerschenkron, in the ensuing years the program faltered, and the essence of the book is the author's attempt to discover the villains who foiled what could have been this great economic "spurt." The book is a tour de force, demonstrating the work of a keen archival detective, and, even more, a brilliant reminder of the vital dictum, which has been much overlooked since the time of Alfred Marshall and his classical predecessors, that economics is primarily political economy. In the course of his inquiry, Gerschenkron traces the many variables, political and economic, involved in the attempt to institute economic policies; the reader thereby gains a number of new insights into the political life of the Dual Monarchy. In the end we learn that one of the prime villains was the noted economist Eugen von Boehm-Bawerk, then the Austrian finance minister, who constantly obstructed the canal-building programs of Gerschenkron's protagonist, von Koerber.

Yet, with all due praise, there are several questions to be raised concerning Gerschenkron's arguments. The first is a question of perspective: von Koerber's program and its apparently remarkable passage through a warring parliament might lose some of their larger-than-life qualities if the readers were told that the hero took office at the onset of a Europe-wide economic crisis which was particularly devastating in Austria-Hungary; yet it is only on page 126 of the 157 pages of text that the readers are apprised of the existence of the crisis. Second, there is the problem of the degree to which von Koerber was or was not the hero of the piece. This, in turn, hinges on two questions. Was the program really a failure? How thoroughly did von Koerber stand behind the program? Although Gerschenkron traces in detail the continuous obstruction and blunting of the canal-building program, his own evidence, as well as that of others, still leaves us with very significant sums being

spent on railroad-building and other parts of the program. Thus the emphasis of the book on Boehm-Bawerk and the canal program may prove misleading. For Gerschenkron, the original program was "seminal" and showed "greatness and originality," but in subsequent years was not pushed forward by von Koerber. Gerschenkron disagrees with critics who have argued that the prime minister was a conservative and nonenergetic autocrat who was thus reticent to fight through a possibly risky fiscal program, yet Gerschenkron and the reader are left without adequate explanations for the seeming failure.

My critique boils down to what was mentioned earlier, the question of perspective. What is crucial is the placing of von Koerber's program in the context of the actual development of the economy. If one takes the view that von Koerber introduced the measures for purely political motives and then acted subsequently in the manner of a typical conservative administrator, letting his program disappear into the maw of the slow-moving and obstructionist bureaucracy, we get one picture. If, however, von Koerber introduced an anticyclical program, quite an innovation at the time, and then in several years there was—as we know there was—a sharp upturn in the economy, the lack of further pursuit by von Koerber of some aspects of his program takes on a quite different light. Whatever the answers, we are left with the usual product of the author's remarkable scholarship, the generation of a new group of provocative Gerschenkronian questions and hypotheses that will set historians working for years to come.

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MARK PHILLIPS. *Francesco Guicciardini: The Historian's Craft*. Buffalo: University of Toronto Press. 1977. Pp. x, 195. \$17.50.

Mark Phillips offers a close reading of Guicciardini's major works, viewing the minor writings as transitional. His approach, one with which I am in agreement, is that Renaissance historians conceived of their task primarily as telling a story within a particular literary genre, and that the narrative techniques employed reveal much about the historian's philosophical assumptions and historical goals. This work is, therefore, a welcome addition to the several recent studies in part or entirely on Guicciardini which the author has not consulted—among them, J. G. A. Pocock's *The Machiavellian Moment* (1975) and my *Francesco Guicciardini* (1976).

Combining textual analysis akin to traditional

literary explication with an examination of Guicciardini's intellectual development, Phillips moves from the early writings (*Storie fiorentine*, *Dialogo del Reggimento*) to an extensive treatment of the *Ricordi* and the *Storia d'Italia*. Of particular interest is his demonstration of how the persona established in the *Ricordi* provides unity and control for this seemingly disconnected collection of maxims. The examination of the *Storia d'Italia*, Guicciardini's historical masterpiece, yields interesting conclusions concerning the writer's treatment of historical change, his interest in literary portraiture (a topic which also characterizes the works of his friend Machiavelli), and his superb narrative style. Phillips defines Guicciardini's style as a controlled and rational formal manner which operates over a strained and complex narrative structure and produces a tenuous balance as well as dramatic tension. Perhaps the most original section of this book analyzes the term *impeto* in the context of Guicciardini's view of social change.

Phillips' work is concerned with style not as an ornament of thought but as its essence. Because of its focus on language, the publisher is to be commended for printing both the original Italian and the translation on the same page. The study of Renaissance Italy is firmly based today upon archival research, as it perhaps should be, but a work such as this one, which attempts to recapture the "pleasure of the text" (as Roland Barthes would put it), that element which originally drew Renaissance writers to the reading and writing of history, should not be overlooked. Perhaps following the example of his subject matter, Phillips presents an elegantly written study, of interest to both the historian and the student of Italian literature and culture.

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PAUL F. GRENDLER. *The Roman Inquisition and the Venetian Press, 1540-1605*. Princeton, N.J.: Princeton University Press. 1977. Pp. xxiii, 374. \$21.50.

Older works, notably those of Reusch and Hilgers, have studied the development of the Index of Prohibited Books in the sixteenth century; in this work Paul F. Grendler has addressed himself to the efforts of the Holy Office to apply it in regulating the book trade in Venice. Venice is of special interest in this connection on several counts: because it was the most important printing center in Italy, because of the prominence of the printing industry in the economy of the city, and because of the participation of laymen representing the government in the procedures of the Inquisition, a



reflection of the jealous and deeply-rooted laicism of Venice. Venice, in short, was a particular test of the effectiveness of the Counter Reformation in Italy.

Grendler has worked carefully through masses of archival material in Venice, Rome, and elsewhere, and presented his findings clearly. The result is valuable on many counts. The book opens with useful surveys of the publishing industry and book trade in Venice and of the organization and procedures of the Inquisition in the city. The author then describes the rise of press censorship there, early efforts to list prohibited books both locally and in Rome, and then a kind of curve in the application of controls, rising for some forty years from the relative indifference of the 1540s and then declining again in the last decade of the century, with the intensification of the jurisdictional disputes between Rome and Venice that came to a head in the interdict of 1606-07. He stresses the collaboration, during the period of censorial zeal (though, with a few exceptions, penalties for violation appear relatively light), of the lay Venetian state with the Inquisition.

Grendler believes, however, that the Venetian case was typical of the response of "Italy" to the Counter Reformation; as he states in his introduction (p. xxiii), Venice "probably acted no differently than any other Italian state." But in the absence of similar studies for other places on the peninsula, this remains only an interesting hypothesis. It is at odds with a widespread impression of both contemporaries and modern scholars that, for political and other reasons, Venice was in important respects rather special—though this impression has also been insufficiently supported by deliberate comparative study. Venice was at the very least, however, in a position to defy the papacy in the closing years of the sixteenth and the early years of the seventeenth century. And if her response to the Counter Reformation was, indeed, typical of Italy as a whole, we would perhaps have to conclude that other parts of Italy would have similarly rebelled against papal supervision during this period if their circumstances had been analogous; nor would I exclude this possibility. But I do not see how Venice could have been both, as Grendler suggests, an enthusiastic participant in a homogeneous Counter Reformation (although this notion is, I suspect, also an illusion, even for Italy), and so resolute an opponent, in what might be described as her moment of truth, of papal claims. There are deeper problems here to which this useful but essentially descriptive book does not penetrate.

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GIORGIO MORI. *Il capitalismo industriale in Italia: Processo d'industrializzazione e storia d'Italia*. (Biblioteca di storia, number 65.) Rome: Editori Riuniti. 1977. Pp. 519. L. 7,500.

Eight of the nine essays in this volume have appeared elsewhere, including one published in English in the *Journal of European Economic History* in 1975. Varying in length, character, and scope, together they constitute an attempt to place the process of industrialization in Italy within a worldwide framework. In evolving his approach Giorgio Mori has made use of a broad literature, Italian and foreign, including such diverse works as Paul Mantoux's *The Industrial Revolution in the Eighteenth Century* and Friedrich List's *The National System of Political Economy*, but above all he has relied upon a reconsideration of the works of Marx and the teachings of Gramsci.

The first essay, "The Process of Industrialization and the History of Italy," serves as a sort of introduction to the volume, setting forth the author's basic premise and placing what follows in perspective. Mori shows himself to be particularly influenced by the assertion of Marx and Engels that large-scale industry creates a true world history insofar as it draws every civilized nation into a world market. He notes that such a market was in existence in 1861 when the Italian kingdom was proclaimed and therefore maintains that one must commence a study of Italian industrial development in the preunification period.

Mori sees the role of the state and of the "mixed banks" in the national economy as being conditioned by the initial subordination of Italian interests to those of the great powers. Likewise he interprets much of the subsequent industrial development in the peninsula in light of the relationship between the national and the world economy. In "The Genesis of Industrialization in Italy" he outlines the underlying assumptions of those who have conducted research and published in the area and then presents his own criteria for such study. These include the need to trace the process prior to unification, to examine it within the broader international framework, and to have it transcend economic movements and the compilation of facts.

Perhaps the most interesting essay is "The Banks, Industry, and Imperialism in the Giolittian Age." Here Richard A. Webster's *Industrial Imperialism in Italy, 1908-1915* is taken to task for its assumption that imperialism, politically understood, is a pure and simple product of state and national policy flowing from an expansive, aggressive industrialism. Mori, on the other hand, is more inclined to interpret this as the Italian manifestation of a worldwide change in the structure of



capitalism in a financial and monopolistic direction. He is also critical of Webster's analysis of the relationship between high finance and industry in Italy—a question that has been largely resolved by the three scholarly and objective volumes of Antonio Confalonieri's *Banca e Industria in Italia*. In large measure the conflict between Mori's and Webster's work reflects the clash of interpretive constructs.

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GIORGIO MORI, editor. *L'industrializzazione in Italia (1861-1900)*. (Problemi e prospettive, Serie di storia.) Bologna: Società Editrice il Mulino. 1977. Pp. 429. L. 8,000.

Although it is frequently said that the influence of Crocean idealism has discouraged economic analysis of society among Italian scholars, the evidence of continued commitment to the writing of economic history is strong. Without going back to the turn-of-the-century school of the *Giornale degli economisti*, it should be sufficient to mention the names of Luigi Einaudi, Gino Luzzatto, Rodolfo Morandi, Francesco A. Repaci, and Umberto Ricci to show that there has been no interruption in Italian economic studies, even during the twenty years of Fascist rule.

Giorgio Mori, who teaches contemporary economic history at the University of Florence, has compiled an anthology of mostly recent writings dealing with the first phase of industrialization after the unification of the country which succeeded in showing, among other things, that the writing of economic history in Italy is still flourishing. To be sure, not all the selections presented by Mori are by Italian scholars for they include such names as Shepard B. Clough, Frank J. Coppa, Herbert Feis, and Sidney Pollard, but Italian names are far more in evidence, including such well-known ones as Giuseppe Are, Luciano Cafagna, Valerio Castronovo, Luigi de Rosa, Stefano Merli, Giorgio Mortara, Rosario Romeo, and Renato Zangheri. The list shows that Mori has made a determined effort to come up with a representative cross-section both with regard to political ideology and methodology of research. Mori himself contributes an original introduction, but all the other writings have appeared previously elsewhere, some being excerpts taken from full-length monographs, others articles from various journals, some of which are difficult to locate in this country.

The first and most encouraging impression conveyed by this anthology is that the historiographical debate over the nature of Italian industrial-

ization is alive and well in Italy today. Mori has arranged the selections under the four general headings of general context, basic traits, history of specific sectors and firms (including silk, cotton, milling, sulphur, steel, coal, and electricity), and the opinions of contemporary observers. Thus Mori has clearly designed the anthology to accomplish several objectives. He certainly succeeds in giving a vivid impression of the range of interpretations, which in these selections range from economic liberalism to various shades of Marxist interpretation. Significantly enough, Mori opens his introductory essay with a reference to Rosario Romeo's attack of 1956-58 on the Gramscian interpretation of the Risorgimento, emphasizing that, although Romeo's attack on the seminal figure of Italian Marxism was highly partisan and probably motivated by the desire to snuff out the debate at its source, the outcome was just the opposite. Although Mori does not pursue his criticism of Romeo very far here, he does so in other writings, including some of the essays he has republished in his *Il capitalismo industriale in Italia* (1977).

In this volume, Mori seems more interested in exploring the debate to ascertain the nature of the consensus than in highlighting conflicts of interpretation. The message is not easy to summarize, but certainly it emerges that Italian industrialization was conditioned, and perhaps determined, by the economic strategy pursued by the greater European powers, that the weakness of consumer demand in Italy compelled Italian industry to rely heavily on various government subsidies and contracts, that low wages compensated in part for the higher costs of mostly imported raw materials, fuels, and semifinished products, and that the process of industrialization intensified regional imbalances within the country. Particularly striking seems to be the generally positive assessment of the role of the state in stimulating industrial growth and expansion into new sectors of production. It should be remembered, however, that the period covered by this volume stops short of the Giolittian age (1900-14) when industrial expansion reached unprecedented proportions. Nevertheless, by 1900 the process of industrialization was clearly underway with all "the upsetting and permanent consequences which it brought to the life of the country" (p. 36).

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ALESSANDRO GALANTE GARRONE. *Felice Cavallotti*. (La vita sociale della nuova Italia, number 25.) Turin: Unione Tipografico-Editrice Torinese. 1976. Pp. xiv, 757. L. 18,000.

Felice Cavallotti, whose career spanned the birth and first three decades of Italian nationhood, was one of the most colorful figures in nineteenth-century Italian politics. The “anti-Caesar poet” and “cavalier of democracy” was probably the best-known Italian politician of his lifetime. In addition to his nearly thirty years in parliament (1873–98) he was also a very successful playwright, poet, writer, and journalist. Most of his artistic work was at best second-rate, but his ready oratorical and lyrical facility, his zest for political polemic, and his outspoken radicalism made him immensely popular in his own lifetime.

As the recognized leader first of the Extreme Left which broke with Depretis and Nicotera after their accession to power in 1876, and then the leader of the democratic radicals, as the Extreme Left gradually sorted itself out into radicals, republicans, and socialists, Cavallotti was an inexhaustible champion of a more democratic, secular, and progressive Italy. Although he was never a socialist, he frequently defended the socialists and anarchists against what he regarded as the arbitrariness of government actions against them. Instead, he insisted on a program of increased democracy in political life and a government which respected the political and civil rights of all its citizens.

A Garibaldian veteran who had fought in the campaigns in Sicily and the South, Cavallotti never changed his ideas. He began as a member of that democratic wing of the Risorgimento which included such figures as Carlo Cattaneo, Giuseppe Ferrari, and the poet Giosue Carducci. Although he was also a participant in the romantic bohemianism of the Milanese *Scapigliatura*, Cavallotti's distinction lay not in the originality of his artistic or political ideas, but in his success at popularizing them through his journalism, his poetry, and his service in parliament. Cavallotti's career abruptly ended in 1898 when he was killed fighting in his thirty-ninth (!) duel, and his death signaled the beginning of the gradual decline of the radical party in the new era ushered in under Giolitti two years later.

Alessandro Galante Garrone's biography provides us with Cavallotti's life in all of its particulars: indeed, no detail, no matter how trivial, escapes his eye. In over eight hundred pages of the most plodding, pedestrian style imaginable, Garrone provides us with almost a caricature of history as narrative. For specialists, the book offers a wealth of detail from the unpublished Cavallotti papers—little of it, however, new in its insights or startling in its conclusions. Since Garrone assumes a casual familiarity with his subject matter on the part of the reader, I suspect that many of the references to personages and events will be lost on

all but a handful of readers. For those who have the time and patience to tackle this work, there is rich detail on Cavallotti and his intellectual and political friends and acquaintances. At times even the constraints of Garrone's style cannot conceal Cavallotti's personal charm and zest for life, and the man whose network of friends extended across the nation and into almost every group in political life—regardless of their differences—seeps through.

Nonetheless, readers who seek a work which “illuminates the social life of his era,” as the series editor promises on the jacket, will be disappointed. This is a workmanlike but undistinguished biography which lends itself well to use as a reference source, but rather less well for the purposes one ordinarily brings to historical study.

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NIȚOARA BELDICEANU. *Le monde ottoman des Balkans (1402–1566): Institutions, société, économie*. London: Variorum Reprints. 1976. Pp. 255.

This is a collection of fourteen articles by the well-known Romanian-French scholar, Nițoara Beldiceanu. The author was and is one of the first Southeastern European historians to assess the impact of the Ottoman Empire on post-1354 Balkan history extensively using Ottoman documents. In the foreword Beldiceanu states that Balkan historians have become sufficiently secure in their national identities to judge Ottoman influences, albeit not always without resentment, on the institutions, languages, and culture of their respective countries. Beldiceanu is a master of Ottoman archives and sources and is thoroughly familiar with relevant Western, particularly French, sources. This is no mean accomplishment. The author's expertise is enhanced further by knowledge of Slavic languages and archives. Ten of the articles in this collection are based on Ottoman documentation.

The author states the view, now accepted by most scholars, that the rapid Ottoman conquest of Southeast Europe was facilitated by conflict and chaos among the peoples of those lands. Secondly, the military might of the Ottomans provided the security indispensable to maintain the new territories. A case in point is the defeat of Bayezit I (1389–1402) by Timur (Tamerlane) at Ankara in 1402, and the “time of troubles” (1402–23) which followed, when the Ottomans were in complete disarray and yet the people of Southeast Europe did not rebel. In fact, many Balkan princes and notables fought with the three sons of Bayezit I who were contending for the throne. Consolidation

of the Ottoman position in the latter half of the fifteenth century encouraged many merchants of the Black Sea and Danube provinces to request integration into the imperial system (see essay no. 7). For the same reason the economic and monetary crises of the sixteenth century and its accompanying disruptions resulted in rebellion and war, as the princes of Wallachia and Moldavia attempted to end their dependence on the Porte (no. 11).

The first six articles deal with the Ottoman campaigns in Moldavia and Wallachia and the role of the Vlachs in Ottoman administration. The remaining articles deal largely with Ottoman legislation regarding mines (no. 9), monasteries (no. 14), taxes on passage of the Danube and the creation of an institution, the *vozarlik*, to effect it (no. 8), and attempts at land reform (no. 13). Beldiceanu puts in relief the workings of the Ottoman Empire and demonstrates the success which the Ottomans had in adapting new laws to existing institutions.

Although specialists in the field will have read most of the articles, and the exorbitant price of Variorum reprints may force the indigent researcher back to the periodical stacks, this book does allow the use of articles from foreign and obscure journals by nonspecialists and those whose libraries have limited periodical holdings.

Scholars interested in a more extensive bibliography of Beldiceanu's works should consult *Turcia*, 7 (1973): 270-71, which lists 43 titles germane to Balkan-Ottomanica. The author's spouse, Irene Steinherr, with whom he collaborates frequently, has 23 major works in the same field. Together the two Beldiceanus have figured prominently among Western and Southeast European scholars who during the last twenty years have revised significantly the historiography of the Ottoman impact on the Balkans during the fifteenth and sixteenth centuries. This volume encapsulates some of that effort.

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ADOLF ARMBRUSTER. *La romanité des Roumains: Histoire d'une idée*. Translated by CIREASA GRECESCU. (Bibliotheca Historica Romaniae, number 17.) Bucharest: Editura Academiei Republicii Socialiste România. 1977. Pp. 279. 22 L.

Adolf Armbruster has traced the development of and testimony to the concept of the Roman origins of the Romanian people from the eleventh to the mid-eighteenth century. After recognizing that no problem in the Romanian past has such a long and polemical history or one so characterized by "errors," the author makes no effort to prove these Roman origins. Instead he simply accepts them as

axiomatic and the starting point for his researches, claiming that they have been "definitively proven." Also, he posits that the Roman antecedents of the Romanians include not merely ethnic continuity, but also the unity of the Romanian people in whatever territorial divisions they might have found themselves, the Latin nature of their language, and the possibility of tracing certain cultural characteristics of the Romanians back to the Roman settlers in Dacia. None of this is particularly original, certainly not Armbruster's contention that the Romanian people's consciousness of their descent from Roman colonists constitutes the dominant collective sentiment throughout their history. Similar statements are to be found in countless articles and books dealing with the "Latinity" of the Romanians.

Armbruster's contribution in the present volume consists in the massive and variegated research he has done in tracking down every conceivable reference to the Romanians' Roman heritage from the eleventh century down to the activities of the great Uniate Bishop Inochentie Micu-Klein during the reign of Maria Theresa. Into an elementary framework of Romanian political and cultural history in Transylvania as well as in the Danubian Principalities, Professor Armbruster has woven testimony from Byzantine authors, Saxon historians, Polish historiography of the sixteenth century, the Romanian chronicles, and the Jesuit missionaries who came with the Habsburg armies to the territories wrested from the Ottoman Empire. It would appear that the author must have discovered every comment ever made by foreigner and Romanian alike up to the mid-eighteenth century regarding the ethnic relationship between Roman and Romanian, the Romanian's awareness of that lineage, and the outside world's recognition of this "Latinity." Overall, this study represents an interesting and extremely thorough analysis of this idea, if we concede Armbruster his original thesis. Despite the fact that he maintains that this French version of his monograph is less polemical than the Romanian original, the presentation possesses familiar and tendentious aspects. This is especially evident in Armbruster's allegation that the sole reason for the lack of Magyar testimony to the Roman origins of the Romanian people during the period under study may be found in the desire of the Magyar noble to sustain his political control over Transylvania and its Romanian population.

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PÁL SÁNDOR. *Deák und die Frage der Hörigen auf dem Reichstag der Jahre 1832-1836*. (Studia Historica Academiae Scientiarum Hungaricae, number 127.) Budapest: Akadémiai Kiadó. 1977. Pp. 94. \$6.00.

This book, published to commemorate the centenary of the birth of Ferenc Deák (1803–76), is not only a commendable work but also a welcome sign that a moderate man of history can be given objective treatment by historians in Hungary. Very surprisingly, it contains almost no archival research but is based instead on excellent contemporary and recent publications from a wide variety of schools of thought, also a positive indication of Hungarian authors' freedom to select their sources as well as their subjects at will.

What then, it may be asked, was the reason for publishing the book, if it contains no new material? One might expect a new interpretation, yet even this is absent. Deák, who never was a very controversial figure or much of a puzzle to historians, emerges as he really was: a conscientious, progressive, liberal reformer from the very beginning of his career. His special interest in achieving the emancipation of the serfs is given proper emphasis. In pursuit of such progressive goals, Deák is shown to have been a clever strategist and political tactician, a statesman whose extensive knowledge of Hungary's constitution and laws gave him a solid foundation for his pragmatic policies. The author quite correctly notes that Deák did not shrink from a tactical retreat in order to make an advance later but never sacrificed his political ideals.

A third of the book is given to appendixes, which contain well-chosen excerpts from Deák's speeches accompanied by cogent and clear editorial commentary. Translation from the original Hungarian was by the masterly pen of Károly Niederhauser.

While the book has nothing new, particularly for those with access to the literature in Hungarian, it does offer a systematic and objective picture of a crucial phase of the Hungarian Reform Era and of the statesmanship of one of the most remarkable figures of the history of the nineteenth century in Hungary. It does so, furthermore, in a Western language in which studies of the period do not abound. The book should be well received.

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GYÖRGY SZABAD. *Hungarian Political Trends between the Revolution and the Compromise (1849–1867)*. (Studia Historica Academiae Scientiarum Hungaricae, number 128.) Budapest: Akadémiai Kiadó. 1977. Pp. 184. \$11.00.

György Szabad, the author of the volume under review and a professor of history at the Eötvös Loránd University of Budapest, is one of Hungary's most respected living historians. The major-

ity of his numerous works deal with the period between the 1840s and 1870s, and they are all highly regarded, even by those who do not quite agree with some of his interpretations. Szabad is known and respected partially for his meticulous research and partially for his competence in dealing with economic and social developments. But he is also known for his special regard for Louis Kossuth and for the whole "independence-orientation" in Hungarian history. His skill and preferences are evident from all of his studies, but especially from his interpretative works dealing with the period between the Revolution of 1848–49 and the Austro-Hungarian Compromise of 1867, including the work under review.

Szabad's *Hungarian Political Trends between the Revolution and the Compromise (1849–1867)* is basically a revised version of his contributions to two well-known multivolume Marxist syntheses of Hungarian history: the four-volume university textbook, and the two-volume popular synthesis edited by Erik Molnár, the late "dean" of Hungarian Marxist historiography. As one would expect, it reflects all of the above-mentioned features of Szabad's scholarship. It is well researched, well balanced, and well written, but, at the same time, it also speaks of the author's preference for Kossuth and Kossuthist policies, including the view that the Compromise of 1867 was simply an ill-conceived agreement between "Hungary's political leaders and the Habsburgs," which benefited only Austria and left most of the basic issues, including the acute nationality question, unresolved.

While one may not fully agree with this interpretation—particularly in light of some of the recent conclusions of a number of Szabad's fellow historians in Hungary about the significant economic and social progress made during the age of Dualism (1867–1918)—one can only applaud his decision to put this well-developed synthesis into the hands of English-speaking scholars.

Various aspects of Szabad's discussion of Hungary's progress toward the Compromise will no doubt be familiar to his readers. But they will learn much about the intricacies of Hungarian party politics before 1867, and they will also enjoy the author's panoramic portrayal of Hungary's social scene of that period, as well as his description of the various political groups and orientations—from collaborators to Kossuthist exiles. Moreover, whatever one's feelings about the author's views on the Compromise of 1867, one cannot help agreeing with his conclusion: "And the Great Powers—who had permitted Hungary's revolutionary struggle against feudalism and absolutism to end in defeat, and then, from 1849 to 1867, denied all genuine support to those Hungarians who sought a democratic alternative—applauded



the Compromise that had been made, only so that barely a generation later, they might take the Hungarian nation as a whole to task for having made such a 'choice' " (p. 167).

As is customary for him, Szabad has again produced an excellent work. Unfortunately, one can hardly do justice to it in a short review of this type. His study is supplemented by a very good bibliography; but, like most Hungarian publications of this nature, it lacks an index.

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JOSEPH A. MIKUS. *Slovakia and the Slovaks*. Washington: Three Continents Press. 1977. Pp. xiv, 224.

The "Roots" phenomenon, which has accelerated the already growing interest in ethnicity in this country, in many ways reflects the romantic nationalism given inspiration by Herder early in the nineteenth century. One of its by-products, most noticeable where smaller nationalities are concerned, has been the re-emergence of volumes such as this one devoted to the glorified presentation of national histories and literatures. Joseph A. Mikus, trained as a lawyer, focuses his attention on the Slovak nation, one of two nations which make up the country of Czechoslovakia. He concentrates on Slovak history (including an essay on historiography) and literature, with shorter sections on language and "personality."

The book is a statement of belief, not a scholarly work. Mikus is sketching his conception of the soul of Slovakia for a popular audience, presumably one made up of Slovak-Americans. He accords to the Slovaks only one belief system, and denies the character Slovak to anyone not holding precisely those beliefs, including persons who favor greater cooperation with the Czechs than he. This is history and culture with a romantic hangover. As did many nationalists of the nineteenth century, he discards the unpleasant aspects, or skips nimbly by them. He observes, for instance, that the wartime Slovak State (which he incorrectly calls a republic) was "not entirely successful in protecting the human rights of some of its individuals or some groups of its citizens" because of foreign pressures (p. 44). In such fashion he skirts the awkward subject of the deaths of tens of thousands of Jews.

In fact, most of Mikus' history is typical Central European journalistic partisanship. The same arguments, making the same misuse of the same figures, have changed little since the political debates of long ago. The sources from which he draws are eclectic—a World War II survey on eye and hair color, a dictionary, a propaganda tract, etc. He gives each the same weight. Even the sta-

tistics usually come second hand. He lumps together nearly all Marxist historiography, finding the same shade of red in a 1954 attack on Daniel Rapant as in the provocative essays by L'ubomír Lipták and Samo Falt'an in the middle 1960s.

The value of the book lies not in the factual material it presents, because that cannot be trusted, but as a statement of the political world view of the Slovak Right. As with his own previous volumes, and those by Joseph Kirschbaum, Gilbert Oddo, and others, many libraries will add this book to their collections because there is "nothing else available" in English. This is a shame, but until Marxist scholars in Czechoslovakia have produced a synthesis of Slovak history in English translation or an English-speaking scholar writes a professional treatment of the subject, there is no alternative.

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LESZEK PODHORODECKI. *Sicz Zaporoska* [The Zaporozhian Sich]. 2d ed., rev. Warsaw: Książka i Wiedza. 1970. 10 Zł.

LESZEK PODHORODECKI. *Żarys dziejów Ukrainy* [Outline of the History of the Ukraine]. In two volumes. Warsaw: Książka i Wiedza. 1976. Pp. 310, 357. 50 Zł the set.

Some of the best work in Ukrainian history is currently done in the Polish People's Republic. Even Poland's president, Henryk Jabłoński, has written a useful book on Polish national autonomy in the Ukraine, 1917-18. Quite naturally, Polish historiography on the Ukraine has concentrated on those aspects of Ukrainian history that have a direct connection with Polish history. Polish historians have therefore paid particular attention to the Cossack period (Horn, Wójcik, Serczyk), to Galicia (Kozik, Hornowa, Najdus, Zaks) and to Poland's Ukrainian minority in the 1920s-40s (Radziejowski, Holzer, Torzecki, Szczęśniak, Szota). Studies of Kievan Rus', the Ukraine in the Russian Empire, and the Soviet Ukraine have been less popular with Polish historians.

What Polish scholars have done in Ukrainian history, they have done, on the whole, well. For one thing, they have better access to sources than do their Western counterparts. Polish libraries and archives teem with Ucrainica, especially with material relating to the Ukrainians in the Polish-Lithuanian Commonwealth, Austrian Galicia, and the interwar Polish Republic. Then, too, thanks to Poland's special relationship with the Soviet Union, the archives and libraries of the Soviet Ukraine are much more accessible to histo-



rians from Poland than to historians from the West.

In addition to certain advantages over their Western colleagues, Polish historians also enjoy some advantages over their Soviet Ukrainian colleagues. Perhaps the most important of these advantages is a more sophisticated historiographical tradition, one continually enriched by intercourse with French and American historical scholarship. This makes Polish historiography, including that on the Ukraine, far more interesting to Western scholars (irrespective of ideological persuasion) than the formulaic output of Soviet Ukrainian historians. Furthermore, Polish censors are a good deal more tolerant than those in the Soviet Union.

The last point, concerning the tolerance of Polish censors, requires qualification. There is no question that Polish historians have a freer hand than Soviet historians. In the books under review, for instance, the author recommends to the reader the works of Mykhailo Hrushevs'kyi, whom he calls "the most outstanding Ukrainian historian." This would be unthinkable in the Soviet Ukraine, where Hrushevs'kyi is anathema. The liberality of the Polish censors, however, is only relative. Books are still occasionally suppressed (notably, in 1973, Szczęśniak and Szota's monograph, *Droga do nikąd*, on the Organization of Ukrainian Nationalists and the Ukrainian Insurgent Army), and all books on Ukrainian topics are carefully edited to avoid untoward offense to the Soviet Union.

The activity of the censor, of course, puts the reviewer in a quandary. Can the reviewer take Podhorodecki to task for all the shortcomings of his popular outline of Ukrainian history? Is the censor's hand visible in the orthodox anti-Normanism and in the absence of any reference to the pogroms of 1881? Are we to complain to Podhorodecki or the censor that this account of Ukrainian history includes no mention of Stalin, nor of the purges, nor of the 1933 famine, nor of the leftward evolution of the Ukrainian Insurgent Army, nor of the dissidents of our time? Small matter who is to blame, if the end product is so marred. Especially for the Soviet period, then, Podhorodecki's volume is unsatisfactory.

The remainder of the outline history is uneven. Podhorodecki's treatment of the archeological period is dry and lacks synthesis. The account of the Kievan and Mongol epochs are long on colorful excerpts from chronicles, but short on stimulating treatment of historical problems. Much better are his sections on Cossack Ukraine and the Ukrainian national revival of the nineteenth century. Fortunately these portions make up half of the work and to a large extent redeem the failings elsewhere.

Podhorodecki's survey is popular history, based on secondary literature rather than primary

sources. Popular history is Podhorodecki's specialty and he has written no less than six other books in this genre. All of these deal at least indirectly with the Ukrainian Cossacks, including his *Sicz Zaporoska*, also under review. The latter work has been almost completely incorporated into the outline history, with only minor changes. *Sicz Zaporoska*, for example, made frequent reference to Sienkiewicz's trilogy, while the outline history does not. The outline history also mercifully deletes such exclamatory sentences as "How different life was then!"

As a popular historian, Podhorodecki is readable, if not profound, and his survey is certainly useful. Lecturers in Russian and East European history will find the section on Cossacks, where Podhorodecki is at his best, a balanced and interesting account of a complex period. The bibliography, which lists recent Polish monographs on Ukrainian history, a chronology, and a modest biographical dictionary contribute to the survey's utility.

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GREGORY L. FREEZE. *The Russian Levites: Parish Clergy in the Eighteenth Century*. (Russian Research Center Studies, number 78.) Cambridge, Mass.: Harvard University Press. 1977. Pp. xi, 325. \$15.00.

Of all the social groups in prerevolutionary Russia, the clergy has probably suffered the most neglect from Western and Soviet historians now doing pioneering work on prerevolutionary society. Western historians have preferred to concentrate on more exciting groups such as the nobility, or officialdom, or army officers, while the Soviets have ignored the church and its personnel for obvious reasons. Now Gregory Freeze has rescued the parish or white clergy from ignominy and in so doing has written a monograph that goes well beyond the narrow subject suggested by the title. It is no exaggeration to say that this volume will be essential reading for all historians interested in the broader questions of church-state relations, the pattern of centralization and state-building in Russia, and perhaps most importantly, the processes of bureaucratization and social change within Russian society as a whole. In my opinion Freeze's book goes a long way toward explaining a crucial feature of prerevolutionary Russian society—its incohesiveness and its capacity for intra- as well as inter-class struggle and animosity, continually fostered by the policies of the central government.

The volume is one of remarkable synthesis tempered by memorable detail. Freeze wisely uses a

thematic approach to relate what happened to the parish clergy when it came into contact with a leviathan-like central state bent on modernization and rationalization (beginning with Peter the Great). He begins with a definition of the parish clergy and its place in both the Muscovite and Petrine polities. After analyzing the effects of the Petrine and subsequent reforms on this social group, the author discusses the evolving relationship between the white clergy and the church hierarchy that itself was undergoing an internal bureaucratization and regularization parallel to that of the secular state. Freeze then moves on to chapters on education and the seminaries, the structure and economics of clerical service, the nature of the parish, and the clergy's relationship to that often forgotten social and administrative unit, and finally, the fate of the parish clergy by the end of the eighteenth century. Briefly stated, this fate was isolation and alienation as a caste whose mores and culture were quite apart from the surrounding secular society. Internally this caste was weakened by economic deprivation and a pervasive sense of cutthroat competition, fear, degradation, and superfluity. Freeze's book makes perfectly clear why the parish clergy during subsequent decades offered no help to the regime in promoting social stability in the countryside, or even to the church authorities in fighting the Old Belief.

One could not possibly reproduce all of the author's insights or even summarize his lucid argumentation in a short review, but a few aspects of the argument deserve mention. Freeze shows that while in the West the eighteenth-century secular state levelled the estates, in Russia the process was the reverse, i.e. the state in its drive to regularize and modernize in fact created estates, in both the clerical and secular "commands." At first under Peter the Great, the state adhered to the traditional Muscovite view of the clergy as a *chin* and made no effort to treat it as a distinct social estate. However, the state quickly began to treat the clergy as inhabitants of an occupied country, frequently resorting to conscriptions into military and civil service and imposition of tax obligations. As the century wore on, the state tried to improve the lot of the clergy, but only by formalistic bureaucratic half measures that failed to address the concrete economic and social problems of the emerging caste. The clerical problem thus remained unsolved until the next major reform initiative of the secular state during the mid-nineteenth century. The only aspect of this general scheme that one might quarrel with is Freeze's continued emphasis on the revolutionary nature of the Petrine state and its successors, and the implication that the spirit of government was qualitatively different during the eighteenth century as

compared to the seventeenth. Although this appears to be true for church-state relations, one would like to know more for comparative purposes about how the clergy was treated by the seventeenth-century administration. Also, one misses further speculation on why the secular government adopted such counterproductive policies toward the clergy at a time when the author portrays the state as a rationalizing and modernizing force.

This work strongly suggests a connection between traditional forms of Russian social organization and the phenomenon of bureaucratization, for one sees a consistent reliance of all parties in the church-state and church hierarchy-clergy relationships on similar principles of formalistic organization, control, and repression.

Finally, one should note the impressive and massive amount of primary (particularly archival) source material that Freeze has used as well as the economy, grace, and wit with which he tells his story. In many ways this is a model for future research in Russian social and institutional history.

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I. P. LEIBEROV. *Tsebel'dinskaia nakhodka: Iz istorii revoliutsionnykh svyazei mezhdru Peterburgom i Kavkazom* [The Tsebel'da Discovery: From the History of Revolutionary Ties between Petersburg and the Caucasus]. Moscow: Izdatel'stvo politicheskoi literatury. 1976. Pp. 222. 59 k.

This book relates selected episodes from the life of the Voronov family over two generations (1850s-1910s). The title is playfully ambiguous in the original and in translation: *Tsebel'da Discovery*. The subtitle helps: "From the history of revolutionary ties between Petersburg and the Caucasus." The book is dedicated to the "revolutionary international friendship of the Russian, Georgian, Armenian, and Abkhazian peoples" (p. 5).

The "discovery" to which the title refers is the massive personal archive of the Voronov family who survived in Tsebel'da down to our own time. The author, I. P. Leiberov, Leningrad scholar and Ph.D. (but apparently unaffiliated with a university or the Academy of Sciences), traced down and employed these materials in writing this book.

There is "discovery," too, in the fact of the family's survival on its estate in Tsebel'da. Tsebel'da is an ancient Abkhazian village that lies on the old military road about sixteen miles outside Sukhumi not far from the eastern Black Sea coast on the piedmont of the western Caucasus. It was in this region that Nikolai Il'ich Voronov was granted over 1,200 acres of land in 1874. The author strikes

a central theme in defense of the Voronov possessions: "Voronov received this apportion of land not in recognition of faithful service to the Romanov throne, but for his personal contribution to the Caucasus region and its peoples, as a prominent social and cultural activist, a scholarly specialist on the Caucasus" (p. 71).

N. I. Voronov was a "nobleman" but, like the vast majority of this "ruling class," was not financially secure. The estate brought almost no revenue. Nikolai depended on income from a teaching post, just as his father had done before him. Although "ennobled," the Voronovs represented that middling social "group" often called *raznochintsy*. Their *locus* in society was not the landed estate or the stock exchange but the "liberal" professions. Most such people disappeared from the historical scene between 1917–38. But Liudmila Nikolaevna Voronova, the youngest daughter of Nikolai, and other members of the family continued to live in Tsebel'da and work in various "liberal" professions, education, pediatrics, and the graphic arts. Liudmila was buried in the shade of a large ash tree on the estate, "Yasochka," in September, 1967.

The book deals primarily with the forty years from the 1870s through the 1905 Revolution. Yasochka became a labor commune briefly in the mid-1880s. After Nikolai's death in 1888, his wife and elder daughter established a peasant school there. The estate served as a sort of post office and rest station for the political opposition in the years prior to 1905. Leiberov makes quite an effort to bolshevize (baptize?) whole crowds of revolutionists and dissidents, and to stretch his evidence all the way to 1917. While not always convincing, these are the episodes which form the central component of the study and which show best the wealth of the Voronov papers.

Leiberov concludes the book with a few words about the two later generations of Voronovs in the post-revolutionary period. However brief and selective, these remarks are as welcome as they are rare in Soviet historiography. One of the most bothersome deficiencies in our understanding of Russian history—as told in the Soviet Union and beyond—is the relative paucity or neglect of opportunities to gauge real change from pre-revolutionary to Soviet times against the experience of uniform institutions, groups, or individuals. To a very large degree the book might best be understood as an effort to expand and enrich the revolutionary tradition. In particular this study reaches back over notable periods of discontinuity—the purges, collectivization, and the Revolution itself—back over a period survived by almost no families like the Voronovs, reaches back for a usable line of continuity.

Popular in tone, the study is nonetheless based on extensive, wide-ranging archival research in Leningrad, Moscow, Tbilisi, and Sukhumi. The archives themselves are like the deeds of the Voronovs. They strengthen the sense of unity of many nationalities within one nation, they broaden the historical "native lineage" of the USSR, and—not to be neglected—they confirm the status of the intellectual social activist, the scholar-intelligent.

This study thus occupies a place somewhere between the wholly secondary popularizations, such as we are familiar with in the "Lives of Noteworthy People" series, and the relatively rare, technical, archival sagas in which the drama of the historical sources becomes a part of the story itself, such as Georgii Shtorm's *Potaennyi Radishchev* (Moscow, 1965). Straddling these two stools, the work falls between. Without knowing the Voronov papers myself but having dealt with several similar collections in the Soviet Union—the Engel'gardt archive, for example—I am "morally certain" that more generous and scholarly citation from the full range of experience and activity down on Yasochka would have given us more critically satisfactory reason to admire the deeds and accomplishments of this fascinating family. We can only hope that more and even better "discoveries" will find their way into contemporary Soviet scholarship.

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E. D. CHERMENSII. *IV Gosudarstvennaia дума i sverzhenie tsarizma v Rossii* [The Fourth State Duma and the Overthrow of Tsarism in Russia]. Moscow: "Mysl'." 1976. Pp. 318. 1 r. 56 k.

For some twenty years Soviet historians have argued among themselves about the politics of the prerevolutionary bourgeoisie. In the past, the oppositionalism of the Russian middle classes was dismissed as meaningless. Soviet scholars usually claimed that bourgeois fear of workers and peasants ultimately outweighed any dissatisfaction with tsarism. Recently, such specialists as Valentin Diakin, V. Ia. Laverychev, I. F. Gindin, and others have challenged this conception. They have chosen to concentrate on previously ignored middle-class elements, especially among commercial and industrial circles, and have drawn a picture that highlights the antagonisms between the autocracy and the various components of the bourgeoisie. One would not be likely to include E. D. Chermenskii in this "new wave." This book, which covers the Fourth Duma (1912–17), treats the question of tsarism's relations with the propertied classes represented in the Duma. It is Chermenskii's most detailed response to the newer con-

ceptions presently being discussed by Soviet historians.

Chermenskii's work is a major revision of his doctoral dissertation, written some thirty years ago, a time when Soviet historiography was somewhat less concerned with independent inquiry. Since then, he has produced several works on the prerevolutionary period and done considerable archival research. As a result, this book is thoroughly grounded in the sources on the period. Chermenskii makes full use of the press and the stenographic reports of the Duma, but it is his extensive exploitation of the archives that constitutes the most useful feature of the book. He is familiar with the funds of the Council of Ministers, the Duma, the various political parties, the royal family, and the Department of Police. Within the police materials, Chermenskii has scored an important archival breakthrough. He is the first scholar to exploit the police department's collection of intercepted and inspected letters, a fund that contains letters from figures encompassing the entire political spectrum, including the extreme and moderate right. Because of the wide variety of materials it contains, this source can have an impact on much research done on the politics of this period.

The richness of Chermenskii's sources is particularly evident in his discussion of the government's campaign against the rights of the Duma during 1913 and 1914. Instead of the usual rumors, we are given precise information on who was behind the campaign, what were their specific thoughts, and what ultimately inhibited their action. This thoroughness aids Chermenskii's discussion of most other issues that confronted the government and Duma during these years.

Yet, despite the high level of scholarship, there are problems of interpretation that should be noted. Quite correctly, Chermenskii downplays the *concrete* significance of the Progressive Bloc, a wartime Duma coalition of liberals and conservatives that sought to force a reluctant and mistrustful government to allow what could roughly be described as public participation in the war effort. He stresses the unwillingness of Miliukov, leader of the liberal Kadets, to push the confrontation between state and society into a potentially revolutionary crisis. As a result, he gives insufficient attention to other, less docile, bourgeois groups such as the Progressive Party. This party drew its support from industrial circles, and in 1916 it withdrew from the Progressive Bloc over dissatisfaction with the weakness of the bloc's opposition to the autocracy. The scholarly debate about bourgeois attitudes then becomes one of emphasis. Was the true representative of the bourgeoisie Chermenskii's Kadets, who drew their strength from professional elements, or was it the

Progressive Party with its base among the industrialists? Space does not permit an answer here. Thus one is forced to fall back on clichés about the need for further research, but it should be noted that the Kadets have received far greater attention from Western scholars, who would do well now to look elsewhere in studying the Russian bourgeoisie.

There is one final interpretational issue worth noting. Chermenskii repeatedly stresses the fundamental unity of aims between the bourgeoisie and the landed nobility, a unity that led both classes to support the autocracy. He argues that this willingness to cooperate was the result of their mutual fear of the laboring classes. Yet if these two propertied classes had truly been united, would a revolution have occurred? No less an authority than Lenin himself posited the disintegration of the ruling classes as the first step of the revolutionary process. In much of Western and Central Europe, cooperation among large industrial and agrarian property holders was achieved. Had a similar relationship existed in Russia, the fate of the February Revolution might have been different. My own sense is that the tensions between the leaders of industry and agriculture were never bridged in Russia. The Progressive Bloc was only a tactical alliance of parliamentary groups. It never led to more profound class collaboration. Given that, the chances of a massive and "uncontainable" revolution were increased.

These interpretational issues should not obscure the scholarly qualities of Chermenskii's work. One can read this book and learn much that is important about a crucial period. It should be read by everyone with an interest in the causes of the Russian Revolution.

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ROBERT H. JOHNSTON. *Tradition versus Revolution: Russia and the Balkans in 1917*. (East European Monographs, number 28.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. vii, 240. \$14.00.

Will experts ever resolve that nettlesome query about Russia's foreign policy being new or old, or a blend of both? In this brief and quite convoluted monograph, Robert H. Johnston of McMaster University begins with a noble objective, i.e., to resolve this historic debate in the context of Russia's policies vis-à-vis southeastern Europe, principally during the period of the Provisional Government. But despite this heroic effort, he fails to



come forward with a neat resolution. His focus is on Paul Miliukov while that noted historian and Kadet Foreign Minister in L'vov's regime from March through May 1917 strove to retain Russia in the war and steadfast to the arrangements negotiated with her allies. The author is, nevertheless, constantly distracted into a myriad of related issues. Whereas virtually each issue has been the topic of many valuable monographs—especially convincing is Rex Wade's *The Russian Search for Peace, February-October 1917* (1969)—the author has vainly attempted, with too little analysis, to distill from many of them a coherent account of Petrograd's hopeless effort to balance war aims, military needs, threatening domestic upheavals, and general bureaucratic incompetence. Rarely will a reader find so pathetic an account as this tale of the absolutely unpardonable dereliction evident among those responsible for steering the Provisional Government, or rather for digging its grave.

Johnston has apparently retraced the route of his predecessors in researching the increasing volume of sources, and he is commended for his thorough notes. But to what avail? The slim book, a new addition to the East European Monographs series from the *East European Quarterly* and issued by Columbia University Press, requires considerable amplification for readers ignorant of the greater issues. Nevertheless, with fewer studies of East Europe now coming off university presses, this monograph is indeed welcome.

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GABRIEL GORODETSKY. *The Precarious Truce: Anglo-Soviet Relations, 1924-27*. New York: Cambridge University Press. 1977. Pp. xiii, 289. \$18.95.

Gabriel Gorodetsky, a lecturer in Russian history at Tel Aviv University, has produced an excellent study of relations between Great Britain and the USSR in the mid-1920s. The author provides a detailed description and a careful analysis of each of the major events in Anglo-Soviet affairs during the period—the abortive treaty of 1924, the Zinoviev letter crisis, the formation of the Anglo-Soviet trade-union alliance, Russian involvement in the British General Strike, the breach of diplomatic relations between London and Moscow, and the Soviet war scare of 1927.

The real strength of Gorodetsky's book lies in its interweaving of the complexities of Anglo-Soviet diplomacy, international trade-union relations, and the struggles of the Comintern with its conservative and social-democratic opponents. Beyond recounting the diplomatic duel which pitted the British cabinet and Foreign Office against the Politburo and Narkomindel, Gorodetsky carefully

traces the tortuous evolution of relationships between the Trades Union Congress, the Labour Party, the National Minority Movement, the British Communist Party, the Anglo-Russian Joint Advisory Council, Soviet trade-union leadership, the Profintern, and the International Federation of Trade Unions. In this respect, *The Precarious Truce* is a valuable companion piece to Daniel Calhoun's *The United Front: The TUC and the Russians 1923-1928* (1976).

Gorodetsky also deserves credit for his sensitive treatment of the domestic political background to both British and Soviet foreign policy. For example, the author shows how the rivalry between S. A. Lozovskii and the Profintern on one hand, and M. P. Tomskii and the trade unions on the other, affected the formulation of Soviet policy toward the British labor movement. Similarly, he explores the differing interpretations of British diplomacy advanced by Foreign Commissar Chicherin and his deputy, Maksim Litvinov. On the British side, Gorodetsky sketches the struggle within the cabinet over policy toward Russia between Foreign Secretary Austen Chamberlain and the "die-hard" (i.e., anti-Bolshevik) faction of William Joynson-Hicks and Winston Churchill. The author is thus able to demonstrate how Prime Minister Stanley Baldwin was gradually drawn toward the "die-hard" position.

Gorodetsky concludes that their "entrenched resentment against communism" led both the Labour and Conservative governments to underestimate the degree to which diplomatic considerations had come to overshadow revolutionary enthusiasm in Soviet foreign policy. Although national security imperatives were rapidly replacing revolutionary aspirations in the Kremlin, Soviet leaders played into the hands of the "die-hards" with the militant rhetoric which they used to cover their retreat from a policy of actively promoting revolutions in Europe. As a result the Soviets met defeat on both the revolutionary and diplomatic fronts.

*The Precarious Truce* is based on an impressive range of sources. The author has utilized the archives of the Foreign Office and the Trades Union Congress in addition to the papers of numerous public figures in Britain. Besides the published Soviet documents, he has also made wide use of British and Soviet periodical and pamphlet literature from the 1920s. Gorodetsky's stimulating book will be read with profit by those interested in diplomatic and international labor history generally, as well as by specialists in British or Soviet history.

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MORRIS ROTHENBERG. *Whither China: The View from the Kremlin*. (Monographs in International Affairs.) Coral Gables, Fla.: Center for Advanced International Studies, University of Miami. [1977.] Pp. xxi, 310.

This volume consists of eleven chapters divided into three parts with a preface and an introduction by the author and a foreword by Mose Harvey. The four chapters of part one center on the Soviet view of the Chinese succession and its aftermath; the two chapters of part two focus on the long-term view which posits an increasingly strong China; and the five chapters of part three deal with Moscow's perceptions of challenges in the world arena and include chapters on the Third World, Sino-Japanese relations, Europe, and Sino-American relations. Generally speaking this is a useful book, bringing together virtually all Soviet writing on contemporary Chinese affairs spanning the period from 1971 to early 1977. For the student contemplating research on the subject this volume will be an extremely valuable bibliographical starting-point. For that purpose alone, it constitutes a contribution to the literature on Sino-Soviet affairs.

The book is not without flaws. The reader encounters difficulty in the first two sentences of the foreword. "This book," Harvey declares, "is not about China. It is about the Soviet Union and what its leaders think about China . . ." (p. ix). The reviewer must take exception to this confusing statement. The subject matter of the book most certainly is China and not the Soviet Union, and it is doubtful that the book actually tells us "what [Soviet] leaders think about China." Rather, it provides an in-depth view of public Soviet commentary on the subject and in this sense only may it be said to be about the Soviet Union.

A second confused argument in an otherwise thoughtful foreword concerns the United States. After going on at great length to make the point that the United States historically has been "as willing, and perhaps as able, as any . . . to play the game of power politics . . . [and] quite adroit in seeking out and utilizing enemies of enemies to what was assumed to be the national advantage" (p. x), Harvey argues the exact opposite in the case of the U.S.-PRC rapprochement. Peking's invitation to President Nixon in 1971, he says, came as a "distinct surprise," the president's earlier signals to the Chinese consisting essentially of "cosmetics, and certainly not . . . a new strategic design aimed at the USSR" (p. xiii). Well, one cannot have it both ways. Harvey echoes the administration's public position at the time, which was to feign ignorance of the strategic implications of the move toward China in order to hinder Soviet perception of the shift. The Washington-Peking alignment, for that is what it is, marks the most

significant structural alteration in global relationships to have occurred since World War II. It is highly unlikely that the Nixon administration did not perceive its possibility or comprehend its implications. Indeed, as author Morris Rothenberg shows in the final chapter of the book, the Soviet Union early and consistently interpreted Washington-Peking ties from just this perspective. As Rothenberg puts it: Moscow "sees this relationship as, at a minimum, providing leverage for the indefinite future to both Peking and Washington in their separate struggles against Moscow, and, at a maximum, inventing a continuing specter of outright collusion between the two against the USSR" (p. 251).

Although the author notes that Soviet writers serve the interests of current Soviet policy, he appears to take the data at face value, failing to distinguish sufficiently between the analytical and the propagandistic. In chapter after chapter what emerges is the consistent tendency of Soviet writers to shape facts to fit preconceived ideological and political propositions. This is commonly understood by specialists in the field and need not be stressed for them, but the failure to make this distinction and to approach the data with a more critical mind tend to weaken the analyses and limit the audience for whom this volume will be useful. In confronting the choice of whether to work for analytical clarity or inclusiveness of data, the author has regrettably chosen the latter.

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PETER VANNEMAN. *The Supreme Soviet: Politics and the Legislative Process in the Soviet Political System*. (Publications of the Consortium for Comparative Legislative Studies.) Durham, N.C.: Duke University Press. 1977. Pp. xii, 256. \$11.75.

The subject of this book is limited in significance, but the treatment is thorough, the tone restrained, and the conclusions judicious. The book is, therefore, a welcome addition to the monographic coverage of the Soviet political system. The timing of publication appears to be unfortunate, for a few months later the new "Brezhnev" Constitution appeared. In fact, changes affecting the principal Soviet legislative body are slight—mainly the lengthening of the period between elections from four to five years. In one respect, indeed, the new constitution supports Peter Vanneman's analysis: he contends that Khrushchev's efforts to heighten the *formal* powers of the Soviet Communist Party had been rejected because of the increasing importance of law; in fact, the provisions of the new constitution enhance the position of the party little if at all.

Vanneman is, of course, entirely aware of the complete *practical* dominance of the party since the foundation of the Soviet state (he unfortunately places the locus of this power in the Central Committee rather than in the Politburo and the Secretariat). From the political standpoint, it is the *potential* of the Supreme Soviet to act as a real legislature which makes a detailed examination worthwhile. Vanneman follows a distinguished tradition (represented by scholars like John N. Hazard and the late Julian Towster) in expressing liberal hopes for such a development. The long delay in issuing the new constitution, together with the very slight alteration in key provisions, suggests that the constitutional route to serious reform in the USSR is a dubious one. If there is any basis for the leadership's evident expectation that such perfunctory changes will satisfy public opinion, how much support for real constitutional government can exist in the Soviet Union? Is it not more likely that other routes—notably toward a traditional modified authoritarianism, perhaps imposed by military authority—will eventually lead to change if the Leninist legitimization wears thin?

While Vanneman is rather optimistic about the prospects for an enhanced legislative role for the Supreme Soviet, he is notably cautious about its potential for satisfying the non-Russian nationalities. As he rightly points out, East Slavic domination of all key positions in the Supreme Soviet virtually precludes nationality protest; various recent developments have further diminished the latter prospect. Given these circumstances, the author is right in stressing other functions of the Supreme Soviet: the enhanced supervisory (*kontrol*) activity of its commissions; an apparent potential for real control over the procuracy, a key Soviet agency; the putative role of the legislative pyramid as a "school for administration"; and, of course, the legitimating role of the Supreme Soviet at home and abroad. Vanneman briefly discusses the relation of the legislature (especially its Presidium) to the Council of Ministers. He does not, however, examine this major institution in detail. This is regrettable since (especially during the 1940s and early 1950s) the Council of Ministers, and for a time the State Defense Committee (both state agencies), really did contest the supremacy of the party. Vanneman does not, however, pretend to provide an exhaustive analysis of the Soviet state or government, much less an extended treatment of episodes which are now long past. Even his treatment of the Supreme Soviet focuses on the last two decades, although he does provide historical perspective adequate for most readers.

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P. G. GRIGORENKO. *The Grigorenko Papers: Writings by General P. G. Grigorenko and Documents on His Case.* With an introduction by EDWARD CRANKSHAW. Boulder, Colo.: Westview Press. 1976. Pp. vi, 187. \$12.50.

One of the most tantalizing questions in the study of Soviet dissidents is why certain persons at the zenith of their professional careers made a public stand against a regime that inexorably swept them into prison camps, psychiatric hospitals, or exile. Many of the activists seem to have been motivated by youthful idealism, generated amid the ferment of the Khrushchev era. Major General Pyotr Grigorenko came out of a different mold.

The character that emerges from this dossier of writings by and about Grigorenko is that of a selfmade man, who risked the laurels he had won in thirty-four years of Red Army service to challenge the pretensions of bureaucrats "nurtured only on ignorance and repression." Grigorenko's voice is that of a patriot expecting the regime to live up to its Leninist promises, of a soldier whose honor makes him scoffingly reply to interrogators that he cannot change his convictions as lightly as his gloves, yet also of an ailing old man frightened of dying obscurely in an isolation cell at Tashkent prison.

The Soviet officials who command the machine geared to grinding an individualist like this into submission are distinguished only by their meanness. Their response to his embarrassing questions about the authoritarianism of Khrushchev is to demote Grigorenko and transfer him to a distant post. When he persists in organizing a group calling itself, in all innocence, "The Union of Struggle for the Revival of Leninism," they find pliant medical experts at the Serbskii Institute of Forensic Psychiatry to certify him insane.

Grigorenko was released from the Leningrad psychoprison in May 1965. Members of the Central Committee of the CPSU privately conceded that his sin was "not in what he said, but that he said it a year and a half earlier than the party said it." Instead of making restitution for fifteen months of incarceration without a trial, however, they left him, former head of the Cybernetics Department at Frunze Military Academy, bereft of rank, pension, and party membership, forced to labor at construction jobs.

During the next four years, Grigorenko became an inspiration to the human rights movement in Moscow. Though in his sixties, his ramrod figure and bald head stood out among the youthful protesters against the preordained verdicts issued in closed political trials. His statements, reproduced here, mirror all the movement's concerns: for civil rights and artistic freedom, for Czechoslovakian

socialism, for the righting of Stalinist wrongs against national minorities.

The KGB operatives outdid themselves in petty harassment of Grigorenko. They searched his flat, seized his papers and typewriter, tapped his telephone, intercepted his mail, tried to cow him by interrogations and to entrap him into contacts with foreign agents. Their round-the-clock surveillance alone, by Grigorenko's ironic estimate, cost the state 200,000 rubles, underscoring his indictment of the secret police as a "parasitic organization, which devours limitless quantities of the people's money," without ever having caught a genuine spy.

The KGB's hand is clearly behind his arrest on charges of "anti-Soviet activities" in May 1969 and his reexamination by the Serbskii doctors to diagnose him paranoid, suffering from "reformist delusions," his "outwardly normal" behavior notwithstanding. Five more years of psychiatric torture follow. The epilogue finds Grigorenko, aged seventy, still an outspoken critic of mindless totalitarianism, recently allowed to come to the U.S.

Despite its skeletal structure and uneven translation, this documentary is essential reading for students of Soviet society as well as for a general audience interested in the remarkable life of a man whose manifest sanity and courage prevail over the persecutory mania of ideological guardians in military and medical uniforms.

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JOEL MOKYR. *Industrialization in the Low Countries, 1795-1850*. (Yale Series in Economic History.) New Haven: Yale Univ. Press. 1976. Pp. xviii, 295. \$20.00.

Investigation into the first phase of industrialization in Europe has long been oriented toward the analysis of single economies. Joel Mokyr has departed from that tradition, moving not in the more obvious direction of comparing economies during similar phases of industrial development but toward contrasting an economy which developed a modern sector of industry with one which, within the same period, failed to modernize. In the later 1790s Belgium had certain advantages which eased the transition from traditional to modern methods of production. Among those Mokyr identifies a large rural or Z-good sector producing textiles and other goods susceptible to being manufactured at lower unit cost, low wage rates, a labor supply in proximity to urban areas and one which could be exploited without an increase in wages, and access to a large and protected market (revolutionary France). Those were not trivial advantages but they would appear, at least at first

glance, to be less promising than those seemingly favoring industrial development in the Dutch Netherlands (well-developed capital markets, a sophisticated commercial structure, substantial savings, an urbanized labor force, and an efficient agrarian sector).

Drawing upon economic theory and theoretical literature, Mokyr fashions a model of early European industrialization shifting from low to high capital-labor ratios. Two matters are emphasized in this application of the model. First, in the Dutch Netherlands wages were substantially higher than in Belgium. That pattern was long standing and a central problem in industrial development in textiles. Dutch historians have customarily stressed labor costs as an inhibiting factor, and Mokyr accepts and refines that judgment while rejecting certain other explanations which have also been advanced to account for Dutch retardation. Second, Mokyr emphasizes capital availability (although not, peculiarly, credit costs). Because the Dutch Netherlands was blessed with a significant supply of capital, it is necessary to explain why that asset was not employed. That quandary is resolved by the claim that there were no links between Dutch capital markets and latent or actual industrial entrepreneurs. Thus an opportunity was missed. But Belgian entrepreneurs financed capital equipment chiefly by using their own resources, following techniques employed in Britain. Had a modern sector of industry been profitable in the Dutch Netherlands, Dutch industrialists should have been able to have financed development by the same means, without using capital markets. That they did not suggests that other factors should bear weight assigned here to absent links with capital.

In a field (particularly with respect to the Dutch Netherlands) in which the body of reliable information in printed sources is still limited, Mokyr chose to interpret the existing record via economic theory, neglecting the potential of archival resources to add new information. Nevertheless, this book is a remarkably lucid example of the assistance which economic theory can give to the demarcation and analysis of problems in industrializing and traditional economies.

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## NEAR EAST

ABDUL HADI HAIRI. *Shi'ism and Constitutionalism in Iran*. Leiden: E. J. Brill. 1977. Pp. xiii, 274. f. 64.

This book will be of great value to students of ideas and politics in twentieth-century Iran and of con-

siderable interest to all students of church and state relations. "Church" in the context of a Muslim country means the Islamic '*ulamā*' (literally "scholars," but usually translated by Iranians as "clergy").

In the political theory of Twelver Shī'ite Islam, which is the state religion of Iran, the only rightful rulers were the Twelve Imāms, and in the absence of the last Imām the '*ulamā*' are his representatives or agents. The Shī'ite clergy have therefore claimed authority not only to conserve and interpret Islamic law (*sharī'at*), but even to approve legislation and to pass judgment on the de facto political regime at any time.

The Qājār regime in Iran was corrupt and oppressive, and too weak to resist foreign encroachment. Many leading '*ulamā*' therefore gave their powerful support to the demand of Iranian merchants and liberal intellectuals for a "house of justice" and ultimately for a parliamentary constitution, which was granted on December 30, 1906. Afterwards a group of '*ulamā*' led by Shaykh Faḡlullāh Nūrī turned against the constitution on the ground that the parliament was claiming sovereign power to enact laws which might be contrary to the *sharī'at*. An attempt at compromise led to the enactment of article two of the Supplementary Constitutional Law of October 7, 1907, which was drafted by Shaykh Faḡlullāh Nūrī and lays down that a committee of five learned '*ulamā*' (*mujtahids*) shall have the power to veto any parliamentary legislation which they find inconsistent with Shī'ite Islam. Despite this, Shaykh Faḡlullāh resumed his opposition on the ground that other articles, particularly article eight (equality of all Iranians before the law) and article twenty (freedom of publication except as regulated by press laws) are contrary to the *sharī'at*. He argued that equality of men and women and of Muslims and non-Muslims is not allowed by the *sharī'at* and that freedom of expression falls within the jurisdiction of the *sharī'at* rather than that of civil statutes and courts.

When Muḡammad 'Alī Shah abrogated the constitution on June 23, 1908, put prominent constitutionalists to death, and ordered troops under a Russian officer to bombard the parliament house, patriotic indignation overshadowed the dissension among the clergy. At Najaf in Iraq, then the main center of Shī'ite theological studies, the leading *mujtahid*, Mullā Kāẓim Khurāsānī, and two other eminent '*ulamā*' condemned this action of the Shah against the Muslim people of Iran. Their messages greatly weakened the position of Muḡammad 'Alī Shah, who was deposed on July 15, 1909. After the restoration of constitutional government, a "people's court" condemned and put Shaykh Faḡlullāh Nūrī to death, and the second parliament enacted laws which were the first steps to-

wards the future secularization of Iran's legal and educational systems; but controversy was again silenced by indignation at the former Shah's attempt to regain his throne and by the Russian ultimatum for the dismissal of the American financial adviser, W. Morgan Shuster, which resulted in the suspension of parliamentary government in December 1911.

Abdul Hadi Hairi begins with excellent chapters on "the intellectual origins of constitutionalism in Iran" and "the Shī'ite '*ulamā*' vis-à-vis the political institution." In these he discusses the ideas of the liberal thinkers Ākhundzāda, Majd ul-Mulk, Si-pahsālār, Mustashār ul-Dawla, Malkam Khān, and Ṭālibov, the teachings of the pan-Islamist revolutionary Jamāl ul-Dīn Asadābādī (known as Afghānī), the history of Shī'ite concepts of the function of the '*ulamā*', the ideas of two outstanding enlightened *mujtahids* Shaykh Hādī Najmābādī and Sayyid Muḡammad Ṭabāṭabā'i, and the attitudes of groups of the clergy in the struggle for constitutional government.

The rest of the work consists of a study of a book entitled *Tanbīh ul-Umma va Tanzīh ul-Milla* and of its author, Muḡammad Ḥusayn Gharavī Nā'īnī (1860-1936). This Persian book was first published at Baghdad in March or April 1909, then lithographed at Tehran in 1910, and reprinted at Tehran in 1955. Nā'īnī, after studying theology at Iṣfahān under Shaykh Muḡammad Bāqir Iṣfahānī, spent almost all the rest of his life at Najaf, where he gained recognition as a learned *mujtahid*. He wrote his book *Tanbīh* in defense of the then temporarily abrogated constitution and in refutation of the absolutist ideas of Shaykh Faḡlullāh Nūrī. After Shaykh Faḡlullāh's execution, Nā'īnī tried to suppress the book. He was among the Shī'ite clergy of Najaf who strongly supported the Iraqi rebellion of 1920 against the British and were subsequently helped or forced by the British to return to Iran. There he joined with Sayyid Abū'l-Ḥaṣan Iṣfahānī (afterwards the leading *mujtahid*) in persuading Riẓā Khān (later Riẓā Shah) to give up the idea of replacing the Qājār monarchy with a republic. After his return to Najaf he made no more pronouncements on either Iraqi or Iranian politics, despite Riẓā Shah's enforcement of measures such as the unveiling of women and the introduction of civil laws and courts in *shar'ī* fields.

In general, Nā'īnī's reasoning in his book *Tanbīh* is based on Islamic premises, except (as Hairi observes) where he uses arguments against despotism derived from the *Ṭabā'i' ul-Istibdād* of the Syrian reformist Kawākibī (1849-1902), who had drawn them from a Turkish translation of the *Della Tirannide* written in 1800 by the Italian poet Alfieri. In the Shī'ite view, parliamentary government is of course not ideal like government by the Imām, but in Nā'īnī's view it is preferable to tyr-



anny, practicable, and necessary for the defense of the Muslim people against internal tyranny and foreign aggression or domination. Nā'īnī thinks that the *mujtahids* may delegate their authority to elected representatives of the Muslim people, even if these representatives are sinful (*fāsiq*), but that the *mujtahids* have a duty (and a right under the above-mentioned article two) to veto any anti-Islamic legislation. He defines equality before the law as the equal liability of all citizens (strong and weak, rich and poor) to prosecution for offenses, but he does not discuss problems arising from the lesser value which the literally interpreted *sharī'at* ascribes to evidence by non-Moslems or from the harsher penalties which in certain circumstances it imposes on non-Muslims. On the other hand, Nā'īnī concedes the right of voting to non-Muslims because they also are taxpayers. Concerning matters such as freedom to express heretical or "godless" (possibly including scientific) views and to abandon or change one's registered religion, or the later very important matter of equal rights for women, Nā'īnī either held traditional Islamic opinions or did not comment. Hairī discusses Nā'īnī's arguments, and while observing that many of them are no more compatible than those of Shaykh Faḡlullāh Nūrī with modern democratic principles, he draws attention to the book's significance as the only reasoned clerical defense of constitutionalism and to its long-lasting influence.

I may be permitted to mention that Iran's "White Revolution" of 1963, in which the most important points were land reform and equality of civic and voting rights for women, encountered opposition from some prominent 'ulamā, notably Rūḡullāh Khumaynī of Qum, and from some religiously minded politicians, notably engineer Mehdi Bāzargān, on the ground that it had been launched without parliamentary approval (even though it had been accepted in a referendum and was to be ratified by the next parliament). It is impossible to know whether the 'ulamā would have approved or disapproved of the land reform and women's suffrage, because they were not consulted. Hairī states that Bāzargān quoted Nā'īnī's *Tanbīh* in his defense when he was put on trial for incitement after the politico-religious riots of June 1963. The theme of Hairī's book is thus still topical in Iran.

The book contains a large number of minor misprints, and its language, particularly in translated passages, is not always fluent. Another criticism concerns Hairī's tendency to identify ideological conflict with conflict over material interests. Although it is true that Iranian constitutionalist 'ulamā and politicians received support from merchants (just as European liberals did from the "bourgeoisie") and that anticonstitutionalists received support from courtiers and "feudalists,"

this does not mean that their ideologies were determined by pecuniary inducements. While many prominent Iranians saved their lives and their wealth by trimming their sails to the shifts of the wind, the constitutionalists Naṣrullāh Biḡishtī Malik ul-Mutakallimīn, Jahāngīr Shīrāzī Šūr-i Is-rāfil, and Šayyid Jamāl ud-Dīn Vā'iz Iṣfahānī, and likewise the absolutist Shaykh Faḡlullāh Nūrī, faced the firing squads of their respective adversaries with unflinching faithfulness to their respective beliefs. The ideological conflict was on a higher than material plane.

The lengthy bibliography and the numerous footnotes will be of great value, particularly as guides to previously unknown Persian sources. Hairī has also done much research in the British Foreign Office archives and has quoted extensively from them.

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#### AFRICA

AFAF LUTFI AL-SAYYID-MARSOT. *Egypt's Liberal Experiment: 1922-1936*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xii, 276. \$14.00.

As the author herself puts it, this is "an old-fashioned book about old-fashioned people" (p. vii)—an atheoretical, narrative, political history. Although it has an introductory chapter dealing with the condition of urban and rural society in Egypt and two concluding chapters which discuss socioeconomic developments and cultural-intellectual trends, the core of the work is its section on politics, which provides a detailed history of Egyptian politics from the nationalist revolution in 1918-19 to the conclusion of the Anglo-Egyptian Treaty of Alliance in 1936.

The book draws on a broad range of new primary material: Foreign Office records on the period, some important unpublished memoirs (the Saad Zaghlul papers but particularly the memoirs of Saad's Wafdist colleague Fathallah Barakat), and conversations with several of the participants in the events described. The result is an excellent microcosmic study of Egyptian politics in the interwar period, thorough and lucid in its untangling of the motives behind major political decisions, critical yet (usually) balanced in its judgments on the major actors involved (the one exception is perhaps the Wafd under Nahhas, where the author may rely overmuch on the testimony of Nahhas' enemies in her negative evaluation). An added dimension of considerable usefulness is biographical; interwoven with the political narrative are detailed and insightful biographical sketches of many of the prominent figures in Egyptian politics. One of the great strengths of the work



is the author's ability to link biography with political history, to relate the decisions made by individuals to the quirks of their background and personality. While the book has little to say about the organizational aspects of Egyptian politics to which Egyptian historians have given so much attention in the past decade, and while it does not deal at any length with the society and life-style of the Egyptian elite, that fascinating blend of French and fallah culture which just as fully as their political programs and actions characterized Egypt's "liberal experiment," it is an important and valuable work on the aspects of Egyptian politics with which it does deal. In sum, an old-fashioned book, but a good old-fashioned book.

Despite the new materials which the study utilizes, its major conclusions about Egyptian political life are largely in accord with those of earlier scholarship. The evaluations of the roles of the main forces—the Wafd's arrogance and intransigence under Saad and its self-centeredness and "extremism" under Nahhas; the huge gap between the principles of the Liberal Constitutionalists and their actions; King Fuad's disastrous impact on political development through his determination to rule rather than merely reign; and Great Britain as the "obviously crucial variable in the Egyptian experience" (p. 168) through its repeated intervention to safeguard its own interests, in disregard of the impact of that intervention on Egyptian political development—are familiar ones, although buttressed here with more documentation than was available in previous histories. Where the work breaks newer ground is in its thesis that the 1919 revolution marked the rise to power of a new class, the native, rurally-rooted elite created by Egypt's nineteenth-century modernization, then in its repeated attempt to demonstrate how the actions of that elite once in power reflected their origins, be it in the fallah-based attitudes and behavior of Saad and the Wafd, the *fahlawi*-tinged (mixed with a goodly dose of nineteenth-century liberal elitism) manipulative approach of the Liberal Constitutionalists, or the peasant cunning and disregard for principle shown by both in their struggle for power. Thus the work's main implicit conclusion seems to me to be not how "liberal" Egyptian politics were, but rather how "Egyptian," how rooted in the traditional attitudes of the people of the Nile, they continued to be even in Egypt's most liberal and Westernized period.

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ARTHUR ABRAHAM. *Topics in Sierra Leone History: A Counter-Colonial Interpretation*. Freetown: Leone Publishers. 1976. Pp. v, 139. 2 leones 50.

In the past twenty years historians of Africa have revised much accepted dogma concerning the colonial period. These twelve short essays (eight of which have been previously published) on aspects of Sierra Leonean history in the last century and half fit this pattern. Most of the essays concern the southern part of Sierra Leone and particularly the Mende, who comprise the largest ethnic group in the country. The notable exception is a chapter on Limba-Koranko relations in northeast Sierra Leone, written by C. Magbaily Fyle. (When one considers that Fyle also coauthored another chapter, it would seem appropriate for his name to appear on the title page.)

Arthur Abraham's essays can be divided into three subject areas, although his arrangement tends to obscure this. One group treats Mende institutions, including slavery and women chiefs. Abraham concludes that slavery among the Mende was similar to that found throughout West Africa, and that it had virtually nothing in common with New World chattel slavery but was a complex institution vital to Mende society, in which slaves worked farms, performed other duties, and were eventually absorbed into the kinship system. According to Abraham women paramount chiefs in Mendeland resulted from British policy in the colonial period and had no precedent before this century. Perhaps the most important section is the chapters which discuss the pre-colonial history of the Mende. In these Abraham shows that Mende political leadership depended on ascribed status and achievement, not the former alone as became the practice under British rule. The author also attempts to establish the existence of nine "confederations of states" in southern Sierra Leone by the late nineteenth century, unlike most writers who maintain that the Mende never developed politically beyond clusters of small villages. The third category of essays concerns the colonial period and includes aspects of the 1898 rebellion against British rule (the "Hut Tax War") and the resulting division of large pre-colonial political units into small chiefdoms. The final chapter concisely states Abraham's contention that the events of the colonial period amply demonstrate the prominent role Africans played in the economic, political, and social changes which occurred.

Some knowledge of Sierra Leonean history would be helpful before reading these essays. One would appreciate more maps, fewer spelling errors, and a more felicitous literary style. Occasionally, Abraham has a disquieting propensity to ascribe unsavory motives to those with whom he disagrees, rather than accepting their work as an honest attempt at scholarly writing. As an example, in speaking of a person who differs with him on the existence of precolonial women chiefs

among the Mende, Abraham describes her as one who "deliberately twists the evidence in a vain attempt to uphold her simplistic thesis" (p. 82).

This book should fulfill, however, the author's hope that it will stimulate research into related subjects not discussed and encourage additional efforts on the topics found herein. It is now the task of historians interested in Sierra Leone to accept this challenge.

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MARTIN STANILAND. *The Lions of Dagbon: Political Change in Northern Ghana*. (African Studies Series, number 16.) New York: Cambridge University Press. 1975. Pp. xiii, 241. \$22.50.

In 1969, at the palace of the Dagomba kingdom, more than sixty people were killed in a succession dispute. The kingdom is a part of Ghana and that state, when it became independent in 1957, inherited the problems of over-rule of a number of such "traditional" kingdoms that the former British colony of the Gold Coast had administered. Legitimacy of claims to the highest office in Dagomba had been troublesome to the colonial government, and have been no less so to the African government.

Martin Staniland investigated the dispute and found himself plunged into precolonial oral history, because recent and current claims to the right to succeed to kingship are based on the precedents in that unwritten history as well as on the subsequent administrative decisions of the colonial and national governments. He treats the evidence for this uncertain past sensitively and, in addition to his analysis of the politics of Dagomba within Ghana, he has produced a notable document on precolonial history. In this he has contributed from his own oral inquiries but has mainly drawn upon the writings of colonial servants, such as H. A. Blair and A. Duncan-Johnston who collected testimony in the 1930s, and he is indebted (as all modern scholars interested in Dagomba are) to David Tait, who extensively recorded oral traditions in the 1950s. Tait's tragic death in a motor accident prevented us from having his interpretations, but the Tait Papers at the University of Ghana have been used by all recent historians of the area.

Staniland's precolonial and colonial history are only background to his study of postcolonial politics, but in actuality there is more space devoted to preindependence history than to recent events. Neglect by colonial historians of the Northern Territories of the Gold Coast makes this volume an important contribution, since Dagomba is one of

the most prominent of the five traditional states in that region.

In less than two hundred pages we have three cameos, each a period piece with its own style, held together not by an artificial setting but by a continuity of descentance. In each, Staniland acquits himself well as a careful craftsman. It may be that the events of 1969 and their consequences in Dagomba will be again retold, but whether or not that happens, we can be sure that the longer span of the colonial period still needs further attention and that the "obscure centuries" of the non-literate period will still tempt investigators to try to discover their secrets. Whenever such efforts are made, there will be a place of respect for *The Lions of Dagbon* in the bibliography.

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RICHARD ELPHICK. *Kraal and Castle: Khoikhoi and the Founding of White South Africa*. (Yale Historical Publications. Miscellany, number 116.) New Haven: Yale University Press. 1977. Pp. xxii, 266. \$17.50.

Richard Elphick has provided a historical and anthropological analysis of Khoikhoi-Dutch relationships in the Cape Colony from 1652 to 1720, characterized by commerce, by disputes over land, livestock, and jurisdiction, and by the cultural decline of Khoikhoi societies. The Dutch not only traded European foods, metals, clothes, alcohol, and tobacco for Khoikhoi sheep, cattle, and labor, but also dispossessed the Khoikhoi of their land and livestock—leading to a series of Khoikhoi-Dutch wars. On the whole, Elphick puts more emphasis on this economic/military basis of the relationship than on the conventional Calvinist/racist explanations. He states, however, that there was practically no cultural assimilation of the Khoikhoi because of the lack of Dutch missionaries and miscegenation, and because of the prevalence of Dutch attitudes of racial and moral superiority. He concludes that by 1720 Khoikhoi societies had disintegrated structurally and culturally because of the ever-increasing economic and military pressures by the Dutch East India Company officials, individual frontier Dutch settlers, and Khoikhoi brokers; and also because of the inherent weaknesses of the traditional Khoikhoi pastoralism, band organization, and nomadism.

One limitation of the book is that it is based primarily upon written European sources recorded during the same period, without corroboration by African traditions. The other limitation is Elphick's inexcusable extensive use of such derogatory and sensitive terms as "tribe," "native," "aborigines," and "Kraal." Realistically, Elphick's scholarly and intellectual redefinition of such terms in order to use them cannot justify the actual insult to

Africans who read the book. It is also questionable that European colonization of southern Africa was begun by thousands of ordinary men, white and brown, quietly pursuing their goals, and unaware of their fateful consequences, as Elphick concludes in the last sentence. The evidence in the book seemingly indicates that the Company, Van Riebeeck, and independent frontiersmen had clear objectives, and systematic means of dispossessing the Khoikhoi of their land, livestock, sovereignty, and civil liberties.

Overall, the book certainly is an invaluable addition to our understanding of South African history. More important, Elphick has provided new details about the traditional Khoikhoi history, politics, and mode of production, as well as their external relations with the "San," Bantu, and pre-Dutch Europeans (1488-1652). The book has broader relevance for the study of frontiersmanship, culture contact, and colonial expansion in other parts of Africa and the world.

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MARTIN CHANOCK. *Britain, Rhodesia and South Africa, 1900-45: The Unconsummated Union*. Totowa, N.J.: Frank Cass and Company. 1977. Pp. xi, 289. \$22.50.

Martin Chanock throws new light on Britain's relationship with Rhodesia, including the disastrous lack of British policy after the breakup of the Central African Federation in 1963. Given the amount already written on Rhodesia, Chanock's is a significant accomplishment. He achieves his result by a careful exercise of the historian's craft: he traces the place and purpose of Rhodesia in British southern African strategy from the end of the Boer War through the aftermath of the Second World War. His book has a thesis. "This study focuses upon the survival of the imperial factor in all the ramifications . . ." (p. 1). It also develops several themes, e.g. "the entire region was a British sphere of influence . . ."; the acceptance by 1910 of "the fundamental southern African premise that while on the one hand there was a shortage of land for Africans, the countryside remained underpopulated"; the Union of South Africa assuming by 1914 the imperial military role of dealing with either African or Afrikaner revolt; and the commitment to territorial segregation of black and white. A final chapter entitled "Argument" re-summarizes the thesis. The result is a well-organized book, which the publisher fortunately has put in an attractive format.

The thesis is convincing. Policy toward the Rhodesian settler, Chanock argues, was never formu-

lated for its own sake. Rhodesia from the early days of the Chartered Company through the Federation was viewed essentially as a counterpoise to something else—either the maintenance of an imperial sphere of influence in the Union or, after 1945, the building of a federal dike against African paramountcy. The dike was necessary because Africans would otherwise extrude those very settlers who had been permitted "a role their strength did not warrant," because Rhodesia had earlier been conceived to be a way of neutralizing the northward spread of nationalist South Africa.

The argument is both more complex and more sophisticated than a short review can reflect. As he develops his thesis, Chanock has insightful remarks about the tenderness of the British government for the Chartered Company; the actual financial record of the Company; Milner and Smuts; the "elder brother" defense policy which South Africa adopted in the mid-1930s toward the rest of British Africa; the development of "native policy" in the Rhodesias as a "model" for South Africa; and particularly the fact that white Rhodesia owed "its existence to a reaction against the growth of Afrikaner power, [and] developed its political identity in a reaction against both British Liberalism and Afrikaner nationalism between 1910 and 1923" (p. 256).

Rhodesia in the end served neither as an internal counterpoise to Afrikaner power within the Union (as it might have, had it been incorporated in 1914 or 1922) nor as an external counterpoise which maintained a British check upon the northward spread of South African ideas. It became instead a British embarrassment—a volatile confrontation area for white and black supremacy. The current situation shows the wisdom and the reality of Chanock's aim—"to put the southern perspective back into Rhodesian history." Without his framework of neoimperialist analysis, the entirely unwarranted strength of the white Rhodesians and the deafness of policy-makers to those African voices which Terence Ranger and others have identified would remain inexplicable. This is an important book, written with verve and clarity.

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ROBIN PALMER. *Land and Racial Domination in Rhodesia*. (Perspectives on Southern Africa, number 24.) Berkeley and Los Angeles: University of California Press. 1977. Pp. 307. \$15.75.

This land alienation study is a major contribution serving Southern Africanists and those interested in last frontiers of settlement and cheap land. Presented as a doctoral dissertation in 1968 at the University of London, it has been thoroughly re-

vised, incorporating recent "development of underdevelopment" studies.

The British South Africa Company's conquest of the Ndebele in 1893 created an indelible balance of power. The settlers appropriated most of the land and established African reserves. White freehold settlement prompted African migration and resettlement. Reserves were conceived as temporary. Africans would be detribalized and then dispersed and their labor incorporated into the economy. Little concern was shown for creating viable reserves. Soil quality, location, water resources, and area were generally inadequate for a growing population. Periodic reapportionment was accomplished without damaging white preeminence or the cheap African labor supply. Integration did not occur. Following the First World War segregation emerged as a protective device and the inadequate reserves became permanent.

Administrative historians will appreciate the descriptions of various commissions of inquiry designed both to please London by demonstrating humane concern for Africans and to satisfy the settlers' natural covetousness. This seemingly impossible task was eased by the careful selection of commission members, by establishing implicit conclusions prior to the inquiry, and through a conspiracy of silence. This study exposes these deceptive tactics.

White survival and economic profitability increasingly led to the involution of viable African agriculture. Robin Palmer adds little to Giovanni Arrighi's earlier contributions, and the author admits the weakest area is the treatment of the African point of view, which doesn't go much beyond T. O. Ranger's *African Voice in Southern Rhodesia* (1970). Palmer's deportation in 1966 made further fieldwork impossible.

The author's use of archival sources, dissertations, and published bibliography is impressive. The frequently repeated chapter orchestration "background, policy and actuality" and the continual presence of an outline format are distracting, however, and the problem of integration becomes more apparent in the twenty-six-page textual appendix.

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HELGE KJEKSHUS. *Ecology Control and Economic Development in East African History: The Case of Tanganyika, 1850-1950*. Berkeley: University of California Press. 1977. Pp. viii, 215. \$13.50.

Historians of Africa, especially East Africa, will be made uncomfortable by this brief volume. For one thing its methodological and interpretative prob-

lems will cause discomfort. Yet this is understandable when we note the author's credentials: he is a political scientist conducting a historical study about economic, epidemiological, and ecological conditions during the nineteenth century. There are bound to be difficulties in such an interdisciplinary undertaking. But he will also cause discomfort in another way. In very explicit terms Helge Kjekshus is protesting the narrow, unimaginative political history that historians of East Africa have produced. In effect he chastises us for having failed to ask meaningful questions and to reconstruct relevant history. What has been produced thus far has failed to enlighten the non-specialist like himself. Hence, Kjekshus has spiritedly filled the vacuum that he perceived by writing his own history. Regardless of the volume's flaws, we can learn from this exercise.

Kjekshus examines fundamental issues that relate to man and his environment in addition to African economic initiatives, particularly during the nineteenth century. A basic premise for the author is that man in precolonial Tanganyika controlled his ecological system. It was, according to Kjekshus, a hostile system, but East African man was winning his struggle with his sound agricultural system, excellent husbandry practices, and his technological prowess, for example in metal working and cotton weaving. This complex economic system provided a wholesome prosperity for East Africans. It enables Kjekshus to claim that the population of precolonial Tanganyika during most of the nineteenth century was relatively stable, or more likely, slowly expanding. All was well in this rather idyllic region of the world until the 1890s when the German colonial regime forced itself upon the populace. At this point the ecological system was dramatically upset—trypanosomiasis and its vector, the tsetse fly, for example, were unleashed—and the population drastically declined. The ecosystem collapsed.

Kjekshus' major contribution is his vivid illustration of the significance of ecological factors, especially the disease environment. (Those who have neglected such issues in analyzing their evidence may want to reassess their previous efforts after reading this volume.) But simply calling attention to a problem does not necessarily imply its resolution. While we historians can certainly learn from Kjekshus about ecological issues to be examined, he, in turn, can certainly learn from historians about their craft.

Take, for example, the use of published sources. Descriptions to support his theses are taken from the works of explorers, missionaries, and early German administrators. After carefully selecting supportive descriptions, Kjekshus then boldly generalizes. Diversity in the evidence is anathema to him. Furthermore, historical changes seemingly



do not occur except along the prescribed lines he has determined until the end of the century when the ecosystem collapses. He portrays for us a static image of East Africans who successfully controlled their environment: famines are pictured as local and seldom serious, warfare never caused serious demographic consequences, epidemic diseases were a nuisance, and the slave trade was only a problem in restricted areas. Observers who claimed otherwise exaggerated; historians who have accepted the exaggerated evidence have perpetuated a myth about the savagery and ineffectiveness of African societies espoused by early colonialists. Kjekshus, of course, proposes the opposite. The population was stable, if not increasing. Agriculture was thriving. Economic activities associated with long-distance trade were peripheral to the entire system and had little effect upon the ecosystem, and so on.

The view presented of the 1800s is—tactfully stated—provocative. The evidence the author selects and presents is simply unconvincing, and, in my opinion, leads to the wrong conclusion. Recent studies of the Haya, Kerebe, Nyamwezi, Ngoni, Pare, Shambaa, and Sukuma are used sparingly. Because his perspective on the nineteenth century is so unrealistic, his assertions for the twentieth century lose much of their impact. This is unfortunate because he has important things to say about man and his environment. He may have, in effect, protested too shrilly and caused more discomfort among the ranks of historians than is actually necessary.

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## ASIA AND THE EAST

BARRY KEENAN. *The Dewey Experiment in China: Educational Reform and Political Power in the Early Republic*. (Harvard East Asian Monographs, number 81.) Cambridge, Mass.: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1977. Pp. xii, 335. \$15.00.

In 1919, at the invitation of some of his former Chinese students at Columbia University, John Dewey extended a tourist visit in Japan into a two-and-a-half-year lecture tour in China. Dewey lectured extensively in Chinese cities to large audiences of students and intellectuals, and his remarks were printed, along with translations of his published works, in Chinese newspapers and magazines. Dewey's visit coincided with the first major expression of modern nationalistic feeling, the May 4th Movement, which takes its name from the date of student demonstrations held to protest

the unfair terms of the Treaty of Versailles (1919).

Dewey's ideas had a major impact on the volatile Chinese intellectual world, an influence equalled only by Marxist ideas following the establishment of the Soviet Union. The discussion of Dewey's visit has become a staple in accounts of twentieth-century Chinese intellectual history, but until now we have not had a monograph which detailed Dewey's China experience, traced the influence of his ideas in China, and evaluated the results of his visit.

Barry Keenan has covered these three areas well in the brief compass of his book. In addition, he provides appendixes which contain complete information about the published Chinese versions of Dewey's lectures—mostly unavailable in English—along with a chronology of Dewey's academic activities in China and a bibliography of Dewey's works in Chinese translation. Because of these strengths, Keenan's work will be of interest, not only to specialists in Modern China, but also to American intellectual historians. They will find themselves unfamiliar with some of the problems and most of the personalities in Keenan's account, but his clear analysis of the impact of Dewey's ideas in China should promote further consideration of the largely unexplored issues of the impact of American philosophers and their ideas on other parts of the world.

Keenan describes how Chinese educators, in a variety of projects, attempted to adapt Dewey's ideas about the school as a vehicle of social reform to the realities of China. The gradualist nature of Dewey's approach, coupled with his inability to develop a means for dealing with the politicization of education in warlord China, meant that Dewey's disciples were unable to make sufficient headway in their hope of creating a stable, democratic polity. Keenan is at his best when dealing with this issue of political power. His conclusion, that "Deweyan experimentalism, as a way of thinking, as a way of acting politically, and as a component of democratic education, offered no strategy his followers could use to affect political power" (p. 161) is sound and will undoubtedly last as an interpretation of the limitation of Dewey's philosophy in the context of early twentieth-century China.

Keenan devotes considerable attention to the issue of educational reform in twentieth-century China, but in this area he underplays somewhat the impact of Dewey. In particular, Keenan discusses the influence of Dewey's ideas on T'ao Hsing-chih, a Columbia University Ph.D. who tried to implement Dewey's ideas as means of accomplishing meaningful political change; Keenan neglects to emphasize how wide an influence Dewey's ideas had within the context of education itself.



The transformation of China's educational system, in urban areas at least, from a Confucian-based program to a distinctly modern one, in terms of teacher training, curriculum, and the attitudes promoted by its graduates, is one of the major changes in China prior to 1949. No single individual or even a single strand of influence produced this modernization, professionalization, and *embourgeoisement* of Chinese education. Yet Dewey's ideas, his sojourn in China, the influence of his writings and speeches, and the work of his Chinese disciples is at the very heart of these changes. No one understood the profound influence of liberal, professionalizing, bourgeois education better than Mao Tse-tung, who, during his lifetime, undertook repeated rectification movements and revolutionary actions in the cultural and educational sphere to correct the influences of Deweyan and other forms of modern bourgeois educational thought. Keenan has taken as his focus the larger issue of how Dewey's philosophy failed to dominate in the political sphere of Chinese life, but in so doing has understated—perhaps inevitably—the great impact Dewey and his disciples had within Chinese education. With this reservation, Keenan's well-written monograph can be highly recommended.

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DICK WILSON, editor. *Mao Tse-tung in the Scales of History*. (Contemporary China Institute Publications.) New York: Cambridge University Press. 1977. Pp. xii, 331. Cloth \$19.95, paper \$5.95.

Certainly Mao Tse-tung was one of the most influential humans to live in the twentieth century, and just as certainly there will long be disagreement over his significance and his accomplishments. This book, appearing little more than a year after Mao's death in September 1976 should provide a base line from which future appraisals can begin. Dick Wilson, editor of the London-based *China Quarterly*, has brought together ten essays of very high caliber by an international group of contributors to his journal, including a number of the leading students of Mao Tse-tung.

The topics, designated according to the many roles which Mao played in a long and diverse career, are as follows: Benjamin I. Schwartz, "The Philosopher"; Stuart R. Schram, "The Marxist"; Michel Oksenberg, "The Political Leader"; Jacques Guillermez, "The Soldier"; Enrica Collotti Pischel, "The Teacher"; Christopher Howe and Kenneth R. Walker, "The Economist"; Frederic Wakeman Jr., "The Patriot"; John Gittings, "The Statesman"; Wang Gungwu, "The Chinese"; Edward Friedman, "The Innovator." Each of these

essays is worthy of detailed discussion but here there is space to mention only a few.

In the first chapters Schwartz and Schram, two of the most eminent authorities on Mao's thought, make careful additions to their scholarship of the last three decades. Schwartz continues his subtle inquiry into the evolution of Mao's thought from a nexus of diverse and shifting Chinese sources and conflicting Western perspectives, and reiterates his view that Mao's Marxist theory tended to be shaped by strategy and not the other way around. Schram finds Mao closer to Lenin than to Marx, especially in his commitment to leadership supremacy, an essential element which Mao was loath to surrender at the height of the Cultural Revolution, but Schram also sees Mao departing from Lenin in altering the notion of proletarian hegemony to fit the conditions of the Chinese revolution by allowing a greater voice to the peasantry. Oksenberg's essay on the political leader is one of the longest and most uneven in the volume. At the same time it is one of the most provocative since it is full of speculation—listing the sources of Mao's power and the qualities of his rule, giving a periodization of the People's Republic according to primary and secondary ministers and organizations, characterizing the tactical side of Mao's political infighting, and much more.

I was pleased to find in Wakeman's essay on the patriot a substantial treatment of Mao's identification with the peasantry, an important topic mentioned by a number of authors but missing from the table of contents. Wakeman, who always tells a story well, approaches the development of Chinese nationalism through the evolution of social classes. Two early traditions of national defense were rooted in class: the loyalism of the Confucian elite to the imperial system and the popular notion of heroic protest and antiforeignism. After 1900 elements of the elite turned to Western education and emerged as a young cosmopolitan bourgeoisie estranged from Chinese traditional culture, whose modernization schemes brought them increasingly into conflict with the overburdened masses. By 1927 Mao identified himself with the popular peasant tradition of protest, later rejecting both elite populism and Westernized bourgeois nationalism. Without attempting to do justice to Wakeman's essay, I will simply object to the approach which subordinates the very important issue of Mao's class identity to the question of patriotism and national identity.

Gittings expresses mixed feelings in the essay on Mao as a statesman. He establishes that Mao early on gave great weight to the importance of external relations for the Chinese revolution. In particular Mao stressed the contradictions among the major powers, whose behavior he judged primarily as it affected China. While recognizing the

service which Mao's statesmanship rendered to the Chinese state, Gittings laments the degree to which Mao's concern with the super powers and China's state-to-state relations overrode concern with—or even involved the sellout of—revolutionary struggles and the fight for socialism in the Third World.

Appropriately enough it is the last essay, on Mao the innovator, which provides the most enthusiastic appreciation of the Chinese leader. Friedman's argument is that Mao's greatest innovations came toward the end of his life. According to Friedman, Mao's great virtue was that he put human beings ahead of the interest of the state, and his greatest innovation was a willingness to seek liberation even in the face of the bleakest imperatives. Thus he could contemplate a policy of self-reliance even at the cost of losing Soviet aid, advocate national liberation in the face of nuclear blackmail, or continue social revolution within China at the risk of holding back economic development. The optimism of Mao's willingness to innovate challenges the rationality of Western political science made pessimistic by its identification with the "centralized, bureaucratic, military-oriented nation state."

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DONALD J. MUNRO. *The Concept of Man in Contemporary China*. (Michigan Studies on China.) Ann Arbor: University of Michigan Press. 1977. Pp. xii, 248. Cloth \$16.50, paper \$9.95.

Most of the books on the educational policies and practices of Maoist China emphasize temporal factors. These policies and practices are perceived as extemporaneously determined by a few leaders. In general, they are condemned as impractical and inhumane. A few recent studies underscore the opposite point of view. The socialist campaign in contemporary China, of which education is an integral part, becomes a humanitarian crusade against injustice, corruption, and elitism. Whereas many books of both persuasions are based on scholarly research, they usually draw upon the most immediate historical documents and rarely go beneath the surface. Moreover, personal likes and dislikes are distinctly manifest in most of these works.

Judged against this background, Donald J. Munro's book has two outstanding features; it is one of a few works on the subject which probe deeply into traditional thinking concerning human nature and the philosophy of education and their transformation in Maoist China. Contemporary educational policies and practices are no longer

viewed as isolated, discontinuous phenomena. They are in important ways manifestations of, though they are not exclusively influenced by, the traditional concept of man. Specifically, these policies have been persistently shaped by the beliefs in the malleability of man and in the significance of man's social nature. Furthermore, Munro analyzes his subject matter in the light of the liberal-democratic idea in America and the Marxist-socialist idea in the Soviet Union. As a result, the thesis of the book emerges more clearly. Unlike many others, Munro does not take Western values for granted, and his comparisons are never weighted toward one side. Munro is not an advocate of any particular cause, yet he does not refrain from making a value judgment. Throughout the book, and particularly in the conclusion, he assesses the strengths and weaknesses of the Chinese (and, implicitly, the American) concept of man and its implications for education. In this sense, the book transcends the boundary of contemporary Chinese thought and history.

Munro's analysis is a potent revision of the influential thesis of Maurice Meisner that Chinese leaders, notably Li Ta-chao and Mao Tse-tung, put an unusual (and perhaps an unrealistic) amount of emphasis on man's will power. The Chinese, Munro argues, implemented policies based on their understanding of human nature, rather than on their immediate personal experiences, such as the Long March, or their psychological promptings. Munro's approach may well open new ground for scholarly research.

This is a mature and balanced book. It should be taken seriously by all who are interested in Chinese and Western theories of human behavior.

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Antioch College

THOMAS C. SMITH. *Nakahara: Family Farming and Population in a Japanese Village, 1717-1830*. Stanford, Calif.: Stanford University Press. 1977. Pp. viii, 183. \$10.00.

In 1955 and 1959 Thomas C. Smith published two books that still profoundly shape our understanding of the course of Japanese economic and social history from the seventeenth to the late nineteenth centuries. Now, twenty years later, he has given us a study in demographic history that will as surely influence the way we think about the agrarian population of preindustrial Japan for decades to come. He is fortunate in his collaborators and his publisher, for the book is in every way a model of clarity of exposition of highly intractable material.

Demographers have long been intrigued by the behavior of population curves for the Tokugawa period (1615-1868). A period of rapid growth in the

seventeenth century was followed by almost total stagnation from 1700 to the end of the period. Smith estimates an annual increase of only 0.03 percent from 1721 to 1846, a figure far lower than that for preindustrial China and most European countries. Yet, he argues, this extended period of static population coincided with a sharp rise in per capita income in Japan, and was not, as is commonly held, a period of stagnation in the economy as well.

Why did the population of Japan remain stable for so long? To find the answer to this familiar question, Smith has gone to the population and land registers for a farming community that he calls Nakahara. There he finds incontrovertible evidence for mortality and fertility rates that are moderate or even low by the standards of the preindustrial West. After a careful consideration of the possible explanations for the low fertility, he rejects all but one. The farmers of Nakahara were practicing sex-selective infanticide: "there was a tendency to use this practice to balance the sexes of the sibling set at each birth order after the second. This suggested that infanticide was not wholly a function either of poverty or of momentary desperation but in part a method of planning the sex composition and ultimate size of the family. The explanation . . . may lie in the relation of family size and composition to farm size and farming efficiency" (p. 14). In Nakahara, then, infanticide was practiced almost equally against males and females, and Smith convincingly demonstrates that there were very good reasons for giving preference to female children under certain circumstances and in certain periods. It is one of the major contributions of the book that the casual allegation so often made that infanticide in Japan meant female infanticide can no longer be sustained.

Throughout the book Smith is at great pains to stress the possibility, which I consider extremely remote, that Nakahara may be an ethnographic curiosity. The best reason for thinking that it is not, in my opinion, is that in terms of this analysis much of Tokugawa population history that has until now seemed bizarre or inexplicable falls into place. It is entirely likely that many of the studies of similar population registers now under way will bear him out. This is a book whose implications carry far beyond the narrow field of Japanese studies, and it should be read with care by all who are concerned with preindustrial societies, the conditions for economic growth, and questions of family planning and resource management.

ROBERT J. SMITH  
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RICHARD VON DOENHOFF, editor. *The McCully Report: The Russo-Japanese War, 1904-05*. Annapolis,

Md.: Naval Institute Press. 1977. Pp. xiii, 338. \$14.95.

As a United States naval officer attached to the Russian Navy during the Russo-Japanese War, Newton McCully observed at first hand the process of Russia's defeat. This report of his year-long stay in the war zone will interest the military historian or student of comparative technology but does not completely satisfy the general reader because of the constraints under which McCully operated and the framework he set for his own observations.

The Russian Navy's treatment of McCully reflected the state of relations between the two governments, soured because of American support of the Anglo-Japanese alliance and American efforts, beginning with the Open Door, to limit Russian expansion in Manchuria. Particularly in Port Arthur, the Russians restricted McCully's movements, refused permission to inspect batteries or damaged ships closely, kept official information from him, and denied him privileges accorded to other attachés. Still, armed with binoculars and a working knowledge of Russian, McCully was able to carry out very detailed military reconnaissance and assess the characteristics and increasingly pessimistic mood of the Russian military.

McCully's report excels in its attention to minute mechanical detail. He fills much of his journal with technical observations, such as the dimensions of boxcars, the nature and extent of damage to Russian ships, comparative sketches of Russian and Japanese torpedo fuses, and a recipe for field-oven sourdough bread. Along with descriptions of officers' personalities, the remainder gives daily accounts of indecisive ship and troop movements (but with no maps provided, these are hard to follow). Twelve appendixes offer further data on fortifications, fleet organization, and changes in command. This type of information reflects McCully's predominant concern with military activity, not the causes or significance of the war. He comes closest to political analysis in discussing the lack of cooperation between the Russian Army and Navy, but never addresses the larger issues of the belligerents' imperial pretensions in China or the interplay between Russia's prosecution of the war and her growing domestic crises. Regarding the outcome, he believes "the root of all their disasters is found in the Russian character" (p. 253), by which he means their lack of independent thought and initiative. His conclusions add little to our general understanding of the conflict, but still, as a primary source, *The McCully Report* provides a lot of fascinating detail for readers to whom the Russo-Japanese War is already familiar territory.

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MORINOSUKE KAJIMA. *The Diplomacy of Japan, 1894-1922*. Volume 1, *Sino-Japanese War and Triple Intervention*. Tokyo: Kajima Institute of International Peace. 1976. Pp. 471. \$32.50.

This is old-fashioned diplomatic history. Essentially it reprints official documents on the Sino-Japanese War and the Triple Intervention so that, in the words of the author, "the pacific intent of [Japan's] diplomacy" may become better known to the world.

The author is a distinguished diplomatic historian who has probably written more books than any other Japanese student of diplomacy. But he belongs to the generation of scholars in Japan (as well as elsewhere) whose primary interest was in reading official documents and constructing a narrative as it emerged from them. Thus one sees events as reported, assessed, and responded to by officials. One gets some idea of differences of views and of power play among a small group of policy-makers, but mostly these are subordinated to the discussion of macroscopic relations among nation-states as monolithic entities. There is virtually nothing on domestic social relations, cultural phenomena, and economic activities as they may define the range of policy alternatives or as they generate pressures upon leaders so that the latter must react as much to them as to external stimuli.

The book is also uni-national. It is heavily based on Japanese sources, and even though it deals with the Sino-Japanese War, it does not examine Chinese or Korean documents. The foreign affairs of these two countries are discussed as they are reported in Japanese dispatches. The same is true of the discussion of the Western powers, although *Die Grosse Politik* is sometimes cited.

The book is not historiographically self-conscious. It does not address itself to various historical controversies. Nor does it refer to important existing studies such as Ian Nish's *Anglo-Japanese Alliance* and the Kims' *Korea and the Politics of Imperialism*. Moreover, the book seems unconcerned with the lively controversy among historians in Japan about the characteristics of Japanese imperialism during the 1890s.

What the author does accomplish is to give a flavor of official Japanese thinking by making available a substantial volume of documents in English. Particularly fascinating are the minutes of the Shimonoseki peace conference where Li Hung-chang and Ito Hirobumi engaged in hard bargaining. These were two senior statesmen trying to outmaneuver each other by using Western concepts of international law as well as by appealing to a presumed spirit of Asian solidarity. These documents are good raw material on the basis of which readers may reconstruct the mentality of Chinese and Japanese leadership at that time.

Does the book show "the pacific intent" of Japanese diplomacy? Only if one accepts the official Japanese view, which the book does, that the war was fought to preserve Japanese security, that the prizes of war were justified to maintain the peace in Asia, and that Japan had nothing but benevolent attitudes toward the neighboring states. One need not be a Chinese or Korean nationalist to question such assumptions. Nevertheless, these were the assumptions which came to be shared by wider circles of Japanese outside the government. There was a social basis to Japanese foreign policy. It is to be hoped that this book will stimulate others to try to relate the few officials it mentions to their domestic constituencies.

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JOYCE C. LEBRA. *Japanese-Trained Armies in Southeast Asia: Independence and Volunteer Forces in World War II*. New York: Columbia University Press. 1977. Pp. iv, 226. \$15.00.

In an earlier publication, Joyce C. Lebra established herself as the leading authority on Japan's wartime alliance with the anticolonial "Indian National Army" of Subhas Chandra Bose. In the present book, the Japanese role in creating similar military groups in Burma, Malaya, the Philippines, Indochina, and the Dutch East Indies is examined by Lebra; in addition, her earlier study of the Indian National Army is summarized. In all of these countries there were patriots who, at least during the initial stages of the Pacific war, regarded the Imperial Japanese Army as more of a liberator than a conqueror. While Japan eventually forfeited much of this initial good will because of arrogance and predacity, it nonetheless played a historic role in Southeast Asia by recruiting and training military elites which, in postwar years, led their nations toward independence and through the nation-building process. Sukarno in Indonesia and Ne Win in Burma are two of the best known postwar leaders in Southeast Asia who unashamedly collaborated with Japan. It is this collaboration which is the subject of *Japanese-Trained Armies in Southeast Asia*.

Regrettably, while there is evidence of first-rate scholarship, the book is marred by serious stylistic problems. Dozens of personalities are introduced, and although the author is to be commended for interviewing many of them, these potentially fascinating characters remain lifeless and wooden. There are far too many passages which are either clumsy, repetitious, or obscure. The whole book is an unfortunate example of the forest and trees problem. The reader is on his or her own when it



comes to separating important theme from illustrative detail. Too much attention is paid to purely formalistic matters: we are given the names of each of the subunits of the Propaganda Subsection of the General Staff's "Special Section" which operated in the Dutch East Indies but are told nothing of the actual activities of the "News Unit" or the "Films Unit," etc. It whets the appetite to learn that Shimizu Hitoshi was in charge of propaganda and that the "most notorious" of all the groups he worked with was a Chinese secret society, the Chin Pang. But, alas, neither Shimizu nor the activities of the notorious Chin Pang are ever discussed.

These criticisms do not invalidate the significance of this germinal study of an important and neglected area of study. They do, however, mean that the book has fallen well short of its potential.

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ROBERT C. HALLISSEY. *The Rajput Rebellion against Aurangzeb: A Study of the Mughal Empire in Seventeenth-Century India*. Columbia, Mo.: University of Missouri Press. 1977. Pp. xi, 119. \$11.50.

This short monograph on the Marwar revolt of 1678-81 is an important contribution to the history of the Mughal Empire. Historians have long seen the Marwar revolt as a watershed in the course of Mughal imperial decline, as the occasion on which Aurangzeb consciously decided to abandon the century-old Mughal-Rajput alliance in favor of a return to an earlier policy of unqualified Muslim supremacy. Thus the revolt itself has been interpreted as a response to Aurangzeb's increasingly hostile attitude toward his Hindu subjects and as a Rajput struggle against Mughal (i.e., Muslim) imperialism. Recently, it has become commonplace for Indian historians to refer to the Marwar revolt as "a war of Rajput independence," "a national war," and even "a people's war." All this Robert C. Hallissey vigorously denies. "The contest between the Rajputs and the Mughals was not a communal confrontation but a struggle between a parochial, traditional political system and an expansionist empire. The Rajputs fought in defense of their clan-dominated political structure, not in defense of their religion. The later participation of the Sisodian ruler in the Mughal service and the appointment of the rebel Rathor leaders to imperial posts indicates the precise nature of the conflict and the inadequacy of interpreting the encounter through a religious perspective" (p. 89).

Hallissey is not alone in challenging traditional assumptions about the nature of the Marwar revolt. Athar Ali in his "Causes of the Rathor Rebellion of 1679" (*Proceedings of the Indian History Con-*

*gress*, 24 [1961]: 135-41) began the task of reinterpretation continued most recently by G. D. Sharma in his *Rajput Polity* (1977). Neither, however, diminishes the value of Hallissey's lucid, carefully-researched case for regarding political and personal interests, rather than communal prejudices and passions, as the motive force for action, whether on the part of Aurangzeb and his advisers, or on the part of the various factions at work in Marwar itself. As he himself declares on his final page: "In attempting to establish thematic continuity between communal tensions in the twentieth century and the Mughal-Rajput confrontation of the seventeenth century, modern historians have overlooked the complexity of Alamgir's [i.e., Aurangzeb's] policy in the Rajput rebellion. Critics of Alamgir have dealt in facile generalizations and ignored the dynamics within the Rajput clan-state and the intricacies of an imperial policy designed to meet specific conditions in Rajwarra" (p. 96). It is to be hoped that the publication of this monograph marks the beginning of a much-needed re-evaluation of the character and significance of the crucial half-century dominated by the inscrutable personality of Aurangzeb.

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WILLIAM J. BARNDT *et al.* *Pakistan: The Long View*. Edited by LAWRENCE ZIRING *et al.* (Duke University Center for Commonwealth and Comparative Studies, number 43.) Durham, N.C.: Duke University Press. 1977. Pp. xviii, 485. \$19.75.

In a volume of fifteen essays written by sixteen American and Pakistani scholars one naturally expects to find a variety of voices, uneven in quality as well as in analysis. Yet the essays here reflect a consistently high level of objective scholarship that extends our knowledge of, and insight into, Pakistan's struggles and achievements.

Concerned largely with background material, economic growth, foreign policy, and political-ideological change, the essays will prove more rewarding to political scientists and economists and less productive for sociologists and humanists who are concerned with the quality of life resulting from the developmental process in Pakistan. The prescription given the reader is further handicapped by the fact that the essays have hardly been updated since completion, mostly in 1974, except for the revision of footnotes. The essays were developed in honor of the late Wayne Wilcox, whose untimely death brought about a memorial symposium.

The authors seek, only partly successfully, to



reproduce a comprehensive analysis of the Bhutto regime and the Pakistan that emerged from the 1971 civil war, stressing contemporary problems and prospects against a background of the country's total experience of independence. They conclude, in general, that Pakistanis can take considerable satisfaction in their achievements since 1971, though these have a certain fragility about them. A more positive constructive theme, other than Islam, is recommended as essential if Pakistan is to overcome its centrifugal tendencies and achieve equilibrium. As for Bhutto's efforts, the authors judiciously plead a lack of perspective and inability to forecast.

If anything, readers encounter a guarded optimism about Pakistan's future; right commitment and right leadership could register successes in spite of other inadequacies or shortcomings in the government's domestic policies. As Wayne Wilcox well put it: Pakistani reality has always been different from Pakistan's dream.

It would be possible to examine the same evidence as the authors and offer different projections. The state of political/socioeconomic development in Pakistan is one of progress and futility, hope and frustration, in about equal portions. On the surface the country has the natural resources to raise general living standards to an acceptable level and to improve the overall quality of Pakistani life. But the inner conflicts of the socioeconomic structure make the likelihood of achieving those goals less certain.

S. M. Burke, W. Howard Wriggins, William J. Barnds, and Norman Palmer investigate shifting opportunities and constraints in Pakistan's long search for a viable foreign policy, a search whose outcome still remains in considerable doubt. The logic of Palmer's argument is convincing: "since its foreign policies were mainly projections of domestic policies and of political experiments which eventually, whatever their temporary successes, did not work, it is hard now to give very high marks to many major aspects of its national achievements."

Ralph Braibanti's essay, "The Research Potential of Pakistan's Development," is among the most challenging. While it is a lament for the virtual omission of Pakistan from scholarly theories of "development," he suggests many fruitful areas for further investigation, e.g., the emergence of multilateral and regional sources of advice, assistance, and inspiration to Pakistan will require "reconstructed modes for regulating, controlling, and correlating these sources with indigenous values in Pakistan." Consequently, Pakistani development now and in the future must deal with a wide variety of ideas, techniques, attitudes, and skills, all attuned to other cultural systems rather

than merely to the American-induced changes of the pre-1971 era.

Two major inadequacies mar the volume; the grossly incomplete index is unworthy of this scholarly effort, and the volume does virtually nothing to prepare the reader for the climactic, fast-paced events of 1977, events which make this work already outdated.

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C. L. M. PENDERS, editor and translator. *Indonesia: Selected Documents on Colonialism and Nationalism, 1830-1942*. St. Lucia, Queensland: University of Queensland Press. 1977. Pp. xi, 367. Cloth \$22.50, paper \$11.99.

English-speaking students of modern Indonesian history are indebted to C. L. M. Penders for the work he has put into this book. Attempting to make key writings on his topic more readily available, Penders has compiled, edited, and translated nearly one hundred documents, virtually all hitherto accessible only to readers of Dutch. Many of these were originally printed in well-known publications, but some, such as letters from the Tan Malaka-Wijngaarden correspondence or Indies police reports of nationalist party meetings, are here made widely public for the first time. Penders' *Indonesia*, however, is more than just a compilation of translations. The documents have been chosen, and the book shaped, to form a companion source-volume to, or perhaps even a modest substitute for, the good history of modern Indonesia that has long been needed.

The first half of the book, "Colonialism," contains documents on the evolution of administrative policy in the Indies; the second half, "Nationalism," is concerned with Indonesian responses. The halves are subdivided into five predictable sections, each introduced by a brief essay intended to place the documents in historical perspective, to bind them together, and to remind readers that sections do not stand independently but instead develop out of one another. This works well in the first half. Here, able essays on culture system, liberalism, ethical policy, decentralization, and education clarify connections between Dutch domestic politics, the condition of the Javanese, and the workings of the Indies bureaucracy. And here most of the documents either were intended originally for public edification or had earlier been collected elsewhere. The second half, on nationalism, is not so strong, however. There are chronological and theoretical reasons why the sections on nineteenth-century anticolonialism, genesis of the modern movement, Islamic politics, Communism,

and secular nationalism do not fit the situation as well as did those Penders used for "Colonialism," but his essays do not go far in exploring why this is so. In addition, more of the nationalism documents have been selected by Penders himself from Dutch archives. Though his choices, clearly, were considered carefully, readers would have benefited greatly from detailed annotation of the documents, which were, after all, official reports and thus meant to serve internal bureaucratic purposes.

Compilers can fault the inclusion or re-translation of some documents available elsewhere in English. Editors can complain about the occasional representation as a single document of a few collected paragraphs which had been widely separated in a more lengthy original paper. And translators can snipe about rare sentences in which only the words, rather than the sentences themselves, have been translated from the Dutch. These faults, however, pale in light of the work that Penders has done. Though he presents no new view of modern Indonesian history, he makes accessible to those interested in that history documents which do much to show why the older view is so strongly held. For this service, Penders deserves thanks.

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NEVILLE MEANEY. *A History of Australian Defence and Foreign Policy, 1901-23*. Volume 1, *The Search for Security in the Pacific, 1901-14*. Sydney: Sydney University Press; distributed by ISBS, Inc., Forest Grove, Ore. Pp. xii, 306. \$27.50.

Analyses of Australian foreign policy have usually concentrated on the years after Pearl Harbor, perhaps after the San Francisco conference on the U.N., rather than on the early federal period when Australia was beginning to formulate views about a separate Australian foreign policy. Research into this earlier period is very thin indeed. The consensus has been that, while there may have been ideas about an Australian foreign policy, no coherent policy emerged until after the Second World War.

Neville Meaney challenges what he describes as "that hazy folk-myth": "From the end of the nineteenth century successive Australian governments, without a distinction of party or person, were aware of their peculiar geo-political circumstances and within the formal framework of the British Empire, they evolved consistent, cohesive and comprehensive defence and external policies to provide for the Security of their own country" (pp. 1-2). Meaney writes as a revisionist historian troubled by the Australian debate over the nature of Australian nationalism, but is himself close to being one of the "new nationalists."

This is an impressive piece of research based on an intensive study of published documents as well as the largely virgin Australian archives and the comprehensive material at the Public Record Office in London. Parliamentary debates and the press in both Great Britain and Australia are also included in this interesting and very thorough piece of work. It is lively, provocative, and meticulously researched. The bibliography and statistical tables are models of their kind.

Australian political leaders and the public at large have always been concerned about the problem of security and the possibility of foreign invasion. Unreal fears of a Russian landing in northern Australia in the 1880s and of a Japanese attack in the two decades before 1914 led to an emphasis on the need for an adequate naval defense. Australia was, after all, twelve thousand miles from London, an isolated Anglo-Saxon outpost close to the continent of Asia. From Alfred Deakin to Joseph Lyons, a generation later, the Pacific rather than the Atlantic was the focal point of Australian defense attitudes and policies. Lyons' Pacific-pact proposals were a reflection of this orientation.

There can be no question that Australian political leaders developed a common concern about the vulnerability of the Commonwealth and an awareness of the difference between their interests and those of Great Britain. But that concern was expressed hesitantly and inconsistently and often with an eye to the local press or elections. The hard fact was that Australia was not equipped with a diplomatic service that could help translate ideas into policy and was usually reluctant to meet the financial costs of an independent defense policy with a separate navy. It was also disabled by an inability to cut the umbilical cord with London. As Barton pointed out in 1901, "Australia could have no 'foreign policy' of its own": pressures for imperial unity in matters of defence and foreign policy conflicted with the desire for Australian independence producing uncertainty and sometimes confusion. Deakin and Fisher may have yearned from time to time for American support to ensure security, but their colleagues were hypnotized by the Whitehall connection.

Australian scholars are indebted to Neville Meaney for his thorough research into Australian defense policy. His task in identifying an independent Australian foreign and defense policy has been a peculiarly difficult one because in the early years of the Commonwealth it "was articulated in an indirect, almost clandestine manner" (p. ix). Meaney is correct in asserting that the Russo-Japanese war of 1905 aggravated Australian feelings of vulnerability, and it did produce a greater willingness to foot naval bills. But he does not demonstrate that "from 1905 to 1914 Australia's

attitude to the world and the empire had undergone a revolution" (p. 257): the response to a European war in 1914 produced a traditional and sentimental gut reaction.

As a revisionist historian, Meaney has rightly drawn attention to the need for re-examining earlier attitudes and conclusions. It is a pity that he has tended to overstate his case and to conclude that repeated criticisms of British policy involved a coherent rather than a fumbling and uncertain attempt to formulate a policy. His emphasis is heavily on defense rather than on foreign policy, on political rather than trade and economic matters affecting those policies. He may change his emphasis in the second volume, to which Australian historians are looking forward.

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RAYMOND CALLAHAN. *The Worst Disaster: The Fall of Singapore*. Newark: University of Delaware Press. 1977. Pp. 293. \$18.00.

"The trouble," wrote General Sir Archibald Wavell, shortly after the fall of Singapore, "goes a long way back." He was right, of course. For all the later recriminations about British defensive tactics in Malaya, about the strategic policy that favored support of Russia and the Middle East over reinforcement of Singapore in 1940 and 1941, and about the unwillingness of Great Britain to maintain a strong defense establishment in the thirties—the trouble, indeed, had its origins in the impact of World War I on global military and economic power balances. As Raymond Callahan points out in his perceptive examination of the causes of Singapore's fall, "the basic fact was that Britain after 1918 was no longer able, and perhaps no longer willing, to defend her world position built up in the Victorian era" (p. 271). He might also have added that, even if Singapore had not fallen to the Japanese in 1942, it would have succumbed in any event to the forces of nationalism within the next decade. Malaya could hardly have remained in British hands while India, Burma, the Netherlands Indies, and the Philippines were gaining their freedom.

The loss of Singapore was a costly military blow and an even more damaging psychological shock. The British defeat has thus, in Admiral Mahan's expression, "crie[d] aloud for explanation," and the body of literature seeking to explain, excuse, or accuse has grown steadily over the years. In retracing the story, Callahan has wisely chosen to focus on the prewar years, rather than on the military campaign itself, which by now has been amply described and dissected. He has made good use of

a number of recently opened documentary sources, notably the personal papers of several senior British military and civil officials, supplemented them with interviews, and has skillfully woven this new material into a thoughtful reappraisal of Singapore's inevitable fall.

*The Worst Disaster* thus serves to flesh out the story told by other writers, especially the official British historians who presumably had access to most documentary sources used by Callahan but whose emphasis and interpretation sometimes differ. For some reason, however, Callahan has not availed himself of the works of American official historians, other than Admiral Morison, so that his discussion of U.S. security interests and strategy in the Far East lacks the depth and balance he displays in analyzing British policy. Equally strange is the absence of Japanese sources—other than the inevitable Colonel Tsuji—the use of which would have strengthened his description of Japanese plans and objectives. It might also have led him to investigate the unfortunate lack of British foresight to plan against Japanese efforts to subvert captured Indian troops. The success of these imaginative efforts may well suggest one of the most important and least discussed aspects of the Malayan campaign: the impact of Western racial attitudes. These attitudes and the forces of Asian nationalism were far more significant to the loss of Britain's Far Eastern empire than the Japanese triumph at Singapore. They warrant a place in any serious examination of this bitter period.

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## UNITED STATES

DAVID HACKETT FISCHER. *Growing Old in America*. New York: Oxford University Press. 1977. Pp. viii, 242. \$10.95.

Despite the burgeoning field of family history, the study of old age has been neglected by historians. David Hackett Fischer's book thus will be welcomed as a first attempt to explore the history of old age in America from the seventeenth century to the present. Fischer seeks "to establish the main lines of change—its pattern, pace, and timing" (p. viii). For his analysis of old age within the context of "a system of age relations," he draws heavily from recent demographic studies for data while also using evidence from literary, linguistic, and material sources, such as portraiture and clothing, with insight and imagination. His use of studies from disciplines other than history—sociology, anthropology, psychology, geriatrics, and geron-

tology—demonstrates the significance of old age for an understanding of a wide range of issues. Fischer has read avidly and assessed this literature thoughtfully, bringing much of it to bear upon his historical concerns and making it accessible through his footnotes to others who will pursue the topic.

After a rapid survey of attitudes toward old age from prehistoric times to the medieval era, Fischer argues that the period from 1607 to 1820 in America was characterized by the "exaltation" and the "veneration" of old age. The period from 1770 to 1820 was marked by a "revolution in age relations" that witnessed a decline in respect for the aged and a rise in the value placed upon youth, a change generally in the direction of equality in age relations. By 1820, however, another transformation in age relations became visible and "gerontophobia became progressively more intense," (p. 101) persisting continuously until 1970. Simultaneously, Americans were embracing a "cult of youth" (p. 132). Beginning about 1910, Americans began to deal with the problems of old age in terms of public policy, gradually establishing a body of welfare legislation that has altered the circumstances of life for many elderly Americans. Fischer, in his conclusion, foresees the possibility of yet another period emerging now that will "create better conditions for old age" and foster "a fraternity of age and youth" (pp. 198–99).

While the periodization set forth by Fischer will provide a useful guide for other historians, the very simplicity of his argument also will invite dissent. Why, for instance, does he neglect to discuss old age in England, indispensable as a background for the assessment of American Puritan attitudes toward old age? If the "exaltation of age" in early America was "a central part of a system of age relationships" that "functioned as an instrument of conservatism in that tradition-bound society," (p. 58) readers might wonder if the newly-created society in New England reflected significant changes in life-experiences compared with old England. Perhaps old age took the immigrants by surprise. Perhaps they had to learn to adjust to the increased longevity of people apparent in many of the first and second generation settlements in New England. Fischer is more intent upon asserting his argument about the veneration of old age than he is in raising a series of unanswered questions about old age during this period. Yet it is precisely through such questions that our knowledge of old age as an integral part of historical experience will be extended.

One important question neglected by Fischer throughout his book is how Americans have faced the process of aging and dying. Death and old age are inseparable, yet death as a subject is notably

absent from *Growing Old in America*. Surely, though, American attitudes toward old age always have been linked closely to attitudes toward death, and religious beliefs, values, and experiences over the generations have provided a major source for a variety of psychological and cultural responses to the final challenge of old age. In trying to discover why Americans were so hostile to the aged for so many generations (if indeed they were), surely psychological and religious factors were as significant as the demographic, economic, social, and legislative factors that Fischer emphasizes.

*Growing Old in America* provides a useful and suggestive point of departure for many future historical studies. Although much remains to be discovered before we can begin to feel that we have a firm grasp upon the historical experiences of the aged in America, Fischer's pioneering study has accomplished its goal of rescuing the subject of old age from neglect. Henceforth, thanks in part at least to his succinct interpretation, the study of old age ought to become an integral part of historical inquiry.

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FRANK SHUFFELTON. *Thomas Hooker, 1586–1647*. Princeton: Princeton University Press. 1977. Pp. xii, 324. \$17.50.

By any fair standard Thomas Hooker was a leading architect of early New England Puritanism. The two basic accounts of his life previously published were those by Cotton Mather (1702) and George L. Walker (1891). Frank Shuffelton's *Thomas Hooker, 1586–1647* is the first modern biography of the man. The author has profited from the wealth of new information and understanding of Puritanism acquired by the last generation of scholars, and unlike his predecessors, he has carefully studied Hooker's published works. Shuffelton describes Hooker's life and ideas in the context of their times, and his sensitive portrait challenges older interpretations at several important points.

The author treats Hooker to 1633, the year he departed for New England, in four chapters. The narrative reveals a life similar to that of many other Puritans of the time. Educated at Emmanuel College, Cambridge, where his conversion also occurred, Hooker established himself as a pastor so successfully as to run afoul of Laud's High Commission. He spent two difficult years as a refugee in the Netherlands before going to Massachusetts. Analysis replaces narrative in two of these chapters. One treats Hooker's ideas on the cure of souls and the other his concept of preparation for salvation.



Hooker's talents found fuller outlet across the Atlantic. We learn that his standards for admission to the church were more lenient than John Cotton's, and that his primary motive for migration to Connecticut from Massachusetts was to escape the disputes that troubled the Bay Colony and to build a more pure congregation. Hooker was no "democrat," but a pastoral rather than a political leader. The Hartford minister was repeatedly called upon to reconcile critical disputes in New England, including the Winthrop-Dudley conflict and the challenge raised by Roger Williams. As the leading preparationist in New England, a subject on which he differed from John Cotton, he played a vital part in the Antinomian Controversy.

The last chapter skillfully assesses Hooker's contribution to New England Puritanism. One legacy was his emphasis on a meditative, intense piety. According to Shuffelton, to speak of a religious declension in seventeenth century New England is misleading, though the modes of expressing holy affections did change. Successors transformed Hooker's preparationist theology, but his tradition of experimental religion and evangelical ministry lived on in New England, especially in the work of Solomon Stoddard and Jonathan Edwards in the Connecticut River Valley. Hooker's more enduring bequest was an image of heroic piety. Ultimately his significance for New England and America was "as a preacher and pastor, as a theologian, and as an architect of the religious community" (p. x).

A few minor errors and inconsistencies mar the book, and in my opinion the author's interpretation is faulty at points, especially on John Cotton's role in the Antinomian Controversy. Nevertheless, the research is thorough, the writing is clear and graceful, and the argument is convincing. This biography provides an excellent understanding of Thomas Hooker and of the Puritanism of the seventeenth century and later.

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ALICE HANSON JONES. *American Colonial Wealth: Documents and Methods*. In three volumes. New York: Arno Press. 1977. Pp. 606; 607-1,471; 1,473-2,216. \$75.00, the set.

This three-volume work reports on a major research effort which has been under way for many years. A second work by Alice Jones, to be published by Columbia University Press, will explain the analytical and interpretive results of the project. The volumes before us now present the principal empirical bases for these results.

Jones has taken seriously the admonition of Raymond Goldsmith that empirical workers should explain their procedures completely, so that the readers of their books may be able to reproduce their results, should they wish to do so. Jones has told virtually all, and, should the reader truly want all, she has offered a computer tape (stored at the Inter-University Consortium for Political Research, Ann Arbor) and extensive handwritten and typed files, deposited at the Newberry Library. But these three volumes alone contain a wealth of empirical material (no pun intended), as well as an extended, thoughtful essay on problems of estimating wealth from probate records. Jones deserves our deep gratitude for assembling these riches and for thinking through so many knotty problems of estimation.

Volume one begins with a short, perceptive, appreciative foreword by Stuart Bruchey. Then Jones takes over, describing with care the types of estate documents she has used in her work and indicating their chief strengths and weaknesses. The remainder of volume one, all of volume two, and the beginning of volume three contain the reproduction of 919 estate inventories (and related documents), constituting a representative sample of estate inventories for all thirteen colonies in 1774. The typical inventory has an identifying number, the name of the decedent, his place of residence, his occupation, his age, the items of property he left (each one valued), and the names of the witnesses to the inventory. About one half of the pages of these volumes is devoted to the reproduction of inventories.

The bulk of volume three explains how the probate documents were used to estimate the wealth of the colonies and the distribution of the wealth among types of wealth, regions, and individual wealthholders. The following difficulties were faced and overcome: 1.) The probate inventories are incomplete. New York inventories are few and far between. Outside New England, land is rarely incorporated in the estate inventory. Evidence on indebtedness is incomplete. Sometimes inventories are not valued. Jones describes how she sought evidence in other documents (e.g., tax lists), and how, as a last resort, she established values by use of statistical regression models. 2.) Probate documents record the property of some fraction of those dying. Ideally, however, we want to know the property owned by all of the living. In order to move from the one to the other, one must know (at least) the ages and sexes of those dying whose estates went through probate; the distribution of the living population among age classes and between sexes; age- and sex-specific death rates; the wealth-holdings of those whose estates did not go through probate. Obtaining the age of each dece-



dent was no mean trick, since testamentary documents do not normally give the age of the decedent. Furthermore, the death rates and age-sex structure of the population in 1774 are unknown, as are the wealth-holdings of those whose estates did not pass through probate.

Jones solved the first problem by genealogical searches and the second by using the age-sex structure of the population in 1800 to stand for the structure of 1774. This was clearly the best available option, although one may have minor reservations as to the relevance of the 1800 structure to 1774. The third problem was solved by using early nineteenth-century death rates, and the last problem was handled by assumption.

Jones explores the pitfalls of each step in her estimating procedure and carefully appraises the results she has obtained. My own strong impression is that her estimates of the total volume of wealth in 1774 and its distribution among regions and among the principal types of wealth are accurate and will prove extraordinarily useful to colonial historians. The distributions of wealth by size of holdings, however, are chancier, and we will want to use them with caution, as Jones advises us to do. In particular, in the period since the publication of these volumes, Jones has decided that the weights given to the components of her southern regional estimate were in error, and for her new book she has altered them. The changes have relatively far-reaching effects on her estimates (reducing the aggregate wealth figures), but these effects are of minor importance, I believe, except in the case of the size-distribution estimates. Here the revised data tend to produce a somewhat more egalitarian picture than do the original figures. But the chief point to notice is that the size-distribution estimates are particularly sensitive to the changes in weights. I believe that they would also be particularly sensitive to changes in, say, the methods of estimating non-probated wealth, and it is for this reason that they should be used with care. Nonetheless, Jones' estimates of the size-distribution of wealth are far and away the strongest and most useful estimates of this type in existence for the Colonial period. Indeed, they may be the strongest American estimates available—apart from those of Lee Soltow for 1860—down to the post-World War I period.

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ROBERT D. MITCHELL. *Commercialism and Frontier: Perspectives on the Early Shenandoah Valley*. Charlottesville: University Press of Virginia. 1977. Pp. xiv, 251. \$13.95.

Robert D. Mitchell presented this monograph initially as his doctoral dissertation in historical geography. The objectives of the study "were to examine how the processes of commercialization influenced land utilization and social evolution in newly settled areas over time and to evaluate the degree to which the emerging structures and patterns may have been modified by considerations of environmental variation, external political control, and different cultural heritages" (p. xi). The study covers the years 1720–1810 and focuses particularly upon settlement patterns, cultural and environmental constraints, land speculation, commercialization, governmental policies, urbanism, social stratification and differentiation, external economic linkages, and the relationships between these processes and institutions. In describing America's early frontiers, Mitchell maintains, scholars have tended to postulate "dual, even dichotomous, modes of economic existence, the 'subsistent' and the 'commercial,' and a typology of discrete stages of self-sufficient, subsistent, and commercial enterprises" (p. 3). He argues, on the other hand, that it is more "realistic" to view eighteenth-century frontier areas "as progressing from a brief phase of primary subsistence and nascent commercialism through various degrees of commercialism and to interpret westward settlement expansion as a process of diffusion of a steadily increasing commercial bias" (p. 4).

After extended analysis of the sources of Valley history, particularly county records and a broad selection of secondary works, Mitchell concludes that land speculation was widespread among both large and smaller landowners, but did not greatly affect the character of agricultural development; although specialty crops emerged, notably hemp, tobacco, and wheat, they were produced within a general framework of commonly-practiced farm enterprises—the valley did not generate a "dual" economy; "the extent and growth rate of external trade were out of all proportion to the limited level of agricultural specialization" (p. 236); the valley was "preindustrial" throughout the eighteenth century; social stratification became particularly perceptible between 1760 and the end of the century; the migration of eastern planters transformed the region "from a socioeconomic extension of Pennsylvania into a western extension of eastern Virginia" (p. 239); and although ethnic distinctions were present, most notably in the case of German groups, they shaped regional development much less than common attitudes concerning the exploitation of regional resources.

Students of agricultural development in the Colonial period lack the detailed sources available to the specialist in the agricultural history of the late nineteenth or twentieth centuries. They are, al-

most of necessity, forced to feats of inference that more fortunate colleagues avoid. On occasion, Mitchell is both artful and ingenious in teasing out conclusions from limited evidence, and he has produced a study that will take its place as an essential item in the bibliography of Colonial and early national agricultural development. There will also, I expect, be some who question the accuracy of some of the author's estimates, regret his inability to sketch the characteristics of the various-sized farm operations in the valley in more detail, remain skeptical of some of his more far-ranging generalizations, and mourn his occasional lapses into fuzzy jargon.

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JAMES A. CLIFTON. *The Prairie People: Continuity and Change in Potawatomi Indian Culture, 1665-1965*. Lawrence: Regents Press of Kansas. 1977. Pp. xx, 329. \$22.50.

This book surveys the general history of the Algonquian-speaking Potawatomi Indians since the appearance of French traders and missionaries on the Great Lakes Plains during the 1630s: the policies non-Indians used to control them, the contacts they made with non-Indian pioneers, and the changes that occurred within their tribe as a result of three centuries of interracial associations. It begins with descriptions of the prehistoric sociology, governance, and philosophy of the tribe, and enumerates changes that appeared in the condition of the Potawatomies during two centuries of intrusions by vanguardsmen representing France, England, and the United States. In those years, tribal members negotiated alliances with interlopers, served as brokers in the fur trade, and worked as mercenaries in non-Indian colonial wars to maintain prominence among the tribes of the Great Lakes region. In the course of so doing, they grew dependent upon trade goods, became subjects of "chiefs" who were submissive to external influences, and lost racial purity through miscegenation. By the outset of the nineteenth century the integrity of their culture was shaken, and the ability of their leaders to resist further change was all but lost.

During the first half of the nineteenth century the unity of the tribe broke down. Leaders of small groups made treaties with the United States and accepted removal, which scattered their constituents from Ontario to Mexico. After that the Potawatomies accepted confinement on small reservations, where they were subjected to techniques designed to prepare them for assimilation into the

mainstream of American society. The acculturation process caused reverses similar to those suffered by many tribes during the last half of the nineteenth century: the allotment of communal land, the sale of surplus acres, the loss of heirship farms, and cultural abuse. By the end of the century the scattered Potawatomies lived from hand to mouth, endured ridicule for their traditional beliefs, and entered a state of psychological depression.

The closing chapters call attention to the fate of the Prairie Band—a small segment of the tribe which finally came to rest on a small tract of land near Topeka, Kansas. Like other conservative Indians, the Prairie people have had to take employment off their reservation in order to survive. Yet they have worked hard to preserve traditional institutions and customs.

James Clifton has prepared a scholarly, interesting, and well-written narrative, but he has attempted to accomplish far too much in a single volume. By dealing with all major aspects of the Native American experience—policy history, contact history, and intratribal history—he has slighted important subjects. His descriptions of recent trends in tribal government are brief, and his remarks about modern social and religious life in the Prairie Band are inadequate, despite the availability of elders in the Band who could have served as rich resources for oral research. Worst of all, his treatment of the history of the Prairie Band is superficial, even though its history is, according to the title, the main subject of the book.

Scholars, students, and buffs will all benefit from reading *The Prairie People*, nonetheless. Though it is somewhat disappointing for the inadequate attention it gives to the past century of Potawatomi history, it provides a glimpse of the adversities that many Indian tribes have faced during the past three hundred years.

HERBERT T. HOOVER  
University of South Dakota

WILLIAM K. POWERS. *Oglala Religion*. Lincoln: University of Nebraska Press. 1977. Pp. xxi, 233. \$11.95.

This is a valuable addition to the large literature on the cluster of North American tribal societies generally, if inaccurately, known as the Sioux. The Oglala, whose religion is the subject of this study, are one of the seven tribes making up the Teton division of the group.

William Powers is largely concerned with the thorny issue of historical continuity and change in Oglala sociocultural forms and practices. His book is a cross-sectional, synchronic study within a lon-

gitudinal, diachronic frame, with a major effort to tap the emic or folk dimensions of Oglala thought added. His main methodological style, leaning much on Claude Lévi-Strauss, is linguistic analysis and modeling. Specifically, he relies heavily on speculative etymological reconstructions of past Oglala (i.e., Lakota) meanings from present phrasings. Powers emphasizes the persistence in unconscious, underlying, unstated, deep "structures" of the Oglala ethos. His basic assumption is that these "structures" consist of a "mental template . . . indelibly etched" on variable overt forms of expression in sociopolitical organization, kinship systems, myth, ritual, and cosmology (p. 21). On the other hand, sociocultural change, some modest degree of which is admitted, is seen as superficial—the shadow on the wall. He makes the point, for example, that as traditional *Tiyospaye* (band) leaders were emasculated politically many turned their attentions and energies to sacred roles in order to retain some measure of meaning, purpose, and power. This example of social change, he concludes, is part of a larger culture historical process, one that is generally recognized by anthropologists: in North American tribes, hard pressed by the development of alien, unitary, agricultural-industrial nation states, once viable sociopolitical institutions and roles have devolved into compartmentalized religious forms. Powers argues, however, that Oglala culture change has been determined mainly by the deep "structures" of the traditional ethos, a process of historical unfolding little influenced by alien pressures. Of course, this conviction about the autonomy of the culture is an important element of a widely popular modern myth. Thus the reader is uncertain whether Powers has revealed a vital Oglala truth or become mesmerized by his own anthropological hocus-pocus.

General readers will find the four chapters on the traditional religion, followed by the summary of Oglala historical experiences, most satisfying. The former are largely based on the older studies of Clark Wissler and J. R. Walker. These chapters consist basically of a neat outline of Oglala religious ethnosemantics. The historical summary is similarly, and uncritically, based on second- and third-hand sources. The few specialists willing to work through the convoluted turgid chapter on Structural Replication may find some satisfaction. The brief essays on the contemporary scene are often superficial. But the concluding essay on Religion and Identity is eloquent and persuasive.

Curiously, in spite of more than twenty years of personal and professional acquaintance with the Oglala and a fluent command of their language, Powers introduces remarkably little of his own, new, original research material and observation

into this study. In the critical synchronic dimension there is little of the rich case material, sample surveys, network analyses, and collection of variant text materials demanded by modern ethnographic standards. It is in the diachronic dimension, however, that the weakness of this research is most apparent. Powers claims that modern Oglala religion replicates the deep structures of an ancient sociopolitical system based on values that have been essentially unchanging since A.D. 1700; this claim, resting largely on etymological speculations, is at best shaky. Like the ethnographers of the early 1900s, Powers seems to have an aversion for ethnohistorical inquiry based on primary sources.

Finally, a bit more concern with history might have protected the author from concluding that the Jesuit father Paul Steinmetz's seeming tolerance for Oglala religious forms was modern and innovative, for this attitude clearly expresses the "holy cunning" the founder of his order dictated centuries ago.

JAMES A. CLIFTON  
University of Wisconsin—  
Green Bay

DEREK PETHICK. *First Approaches to the Northwest Coast*. Vancouver: J. J. Douglas; distributed by University of Washington Press, Seattle. 1976. Pp. 232. \$12.50.

This deceptively slim volume contains a concise history of the exploration of the coastal area of the Pacific Northwest, centering on Nootka Sound, during the half century prior to George Vancouver's epic charting expeditions of 1792–94. The author begins with a broad overview of the age of discovery up to the early eighteenth century and then deals successively with: the Russian advances of the 1700s, the Spanish voyages northward from California during the same period, the explorations of James Cook, the early beginnings of the fur trade in the Northwest, the emergence of the Americans in the Pacific Northwest trade, and the conflicting international claims on the Northwest that culminated in the Nootka Sound controversy.

It is entirely too much for a book this brief. Although obviously writing for the general reader rather than the professional historian or, indeed, even the well-read layman, the author simply tries to cover too much ground too fast. The tone is set in the opening chapter in which explorations as far apart in space and time as the Portuguese circumnavigations of Africa, the Dutch expeditions in the South Pacific, and the British, French, and Spanish discoveries in the interior of North America are handled with no more systematic outlook than one

lent by simple chronology. Nor does the narrowing temporal and spatial perspective of the succeeding chapters do much to make up for the absence of a central theme or approach, or for the tendency to paint with an extremely broad brush. For example, the story of the real "first approaches to the Northwest Coast" made by Vizcaino and others during the sixteenth and seventeenth centuries is generalized to "exploratory voyages up the western coast of North America had been made several times" (p. 35).

Despite these criticisms, the book has something to commend it to those readers for whom the general overview is preferred. The dates and directions of expeditions are accurately presented, and the explorations of the Russians, French, British, Spanish, and Americans are discussed as part of the greater historical realities of the times in which they occurred. Appropriate emphasis is placed on the journals of explorers themselves, and each chapter contains lengthy citations from firsthand accounts, especially with regard to the presentation of ethnographic data. Unfortunately, both the exploration and ethnography are somewhat romanticized: the explorations of Russia and Spain become "the probing tentacles of two great empires" (p. 53) and the native Americans of the Northwest become "savage whale hunters" with "dark eyes" (p. 168). Such hyperbole may be forgivable in a nonscholarly work. What is not forgivable in any kind of work which deals with exploration is the lack of maps. With the exception of one very small scale simple line drawing of the coastal area from Puget Sound to Kodiak Island, the book does not contain a single map. The limitations of the book are perhaps summed up and symbolized by this singular omission.

JOHN L. ALLEN  
*University of Connecticut*

JAMES WEST DAVIDSON. *The Logic of Millennial Thought: Eighteenth-Century New England*. (Yale Historical Publications, Miscellany, number 112.) New Haven: Yale University Press. 1977. Pp. xii, 308. \$17.50.

The author's primary concern in this study is to provide an analysis of the way in which pre-Revolutionary New England made use of the apocalyptic tradition. The book is welcome because despite close attention given to the Great Awakening and Jonathan Edwards, eighteenth-century millennialism is probably less well understood than the Puritan millennialism of the seventeenth century or the rival pre- and postmillennial themes of nineteenth-century American religion. Not only were New

England clergy attentive to English scholars' labors accommodating eschatology to the Enlightenment, but they themselves produced apocalyptic commentary of bewildering complexity and variety. Conflicting schemes of historical chronology, paradoxical combinations of optimism and anxiety, and diverse expectations concerning God's use of natural or supernatural means abounded throughout the century. John West Davidson gives full scope to this variety and concludes that previous historians—Goen, Tuveson, Heimert, and others—have inadequately explored the difficulties of interpretation and discovered shifts in eschatology which are not justified by the evidence.

Instead he argues that from Increase Mather to Timothy Dwight New England apocalyptic thought displayed considerable continuity and a peculiar consensus. Behind the preachers' differences lay a common reliance on millennial doctrine to present an instructive "afflictive model" of historical progress. The Lord of history was leading his people to an ultimate salvation through affliction and judgment. Indeed, "the pattern behind the grand history of the Revelation" was "the same pattern which shaped the new birth of every believer" (p. 129), and the millennial scheme could be perceived as the "social application of the conversion model." Until the century's end emphasis fell less on the glorious consummation of history than on the assurance of divine governance, through a providential balance of mercies and judgment, in the historical process—an assurance which gave encouragement and comfort amidst repeated crises of the later Colonial era.

A pastoral function of millennial teaching is not, of course, unique to eighteenth-century New England; it was probably a more classic pattern for the doctrine than its dynamic and revolutionary role in the seventeenth century or the optimistic cultural assimilation of a later day. But while Davidson's investigations introduce a cautious and conservative note into our assessment of the evolution of New England tradition, his interpretation appears to provide the best basis for unifying a complex religious genre, just as his examination of the use of the "afflictive model" in the Awakening controversies, Anglo-French colonial struggles, and American Revolution fixes more exactly millennialism's limited historical influence. If the book has occasional disappointments, they seem related to its organization and focus. A thematic approach and a trial and error method of reconstructing millennial logic have produced some repetition, while an introduction which summarizes the Book of Revelation and surveys the century through representative men—Mather, Edwards, and Dwight—makes a rather slow start. Rigorous limits have also left the study with no satisfying



anchor in early Puritan teaching, few allusions to dissent (Isaac Backus receives only footnote mention), and scant reference to important colonial evangelicals outside the province. In contrast, New England's dependence on contemporary Anglican, Nonconformist, and Scottish learning is carefully documented and explained. Indeed, the book so copiously reveals that the preachers were a part of a broad transatlantic community of discourse that one regrets that the study's definition did not allow discussion of New England's place in the larger context of eighteenth-century Anglo-American eschatology. The loss may be especially notable in the description of the shift to postmillennial conviction in the early republic which, while intriguing, seems not only too brief and speculative but also too parochial to contribute materially to the history of the idea.

Nonetheless, the author's achievement is impressive. He has given us a most careful examination of New England's eighteenth-century millennial literature, correcting previous generalizations, revealing nuances and subtleties of doctrine, and clarifying how that doctrine functioned in personal and corporate life in the province.

J. F. MACLEAR  
University of Minnesota,  
Duluth

GEORGE G. SUGGS, JR., editor. *Perspectives on the American Revolution: A Bicentennial Contribution*. Carbondale: Southern Illinois University Press, for Southeast Missouri State University. 1977. Pp. xiv, 141. \$7.95.

Between 1972 and 1976 the Southeast Missouri State University sponsored lectures on the American Revolution by five important historians. The lectures, presented to general, mostly undergraduate, audiences, are well-written, never patronizing, and clear presentations of their authors' arguments.

Now, edited and introduced by George Suggs, the lectures have been printed for a wider audience. One wonders if this was necessary; good lectures are not simply papers given with an eye to publication, and of the five presented here, only Robert Brown and Jack Greene knew that their lectures would be printed. None of them say much that scholars will find new, though they do clearly present the points of view that made their authors important historians.

Robert E. Brown's "Did the American Revolution Really Happen?" is the poorest of the lot. It is almost a period piece from the 1950s, a simplification of Brown's already simplistic attack on the Progressive historians Beard, Becker, Schlesin-

ger, Jameson, Brinton, and Jensen. He tastelessly equates them with the Watergate gang, though he convicts them only of the "crime" of finding class conflict in early America. He dismisses the New Left in a couple of sentences on the grounds that they disagree with each other. Brown mentions nothing published since 1967. Printing this lecture at least serves the purpose of exposing the poverty of Brown's thought.

Carl Ubbelohde's "The Idea of Independence" may have little for the scholar, but it is a marvelous lecture for a general audience and ought to be widely reprinted. Clearly and unpedantically, Ubbelohde discusses the agonizing questions Americans had to face between early 1774 and mid-1776 and reminds us that "the idea of independence is central in all of the annals of and interpretations about the American Revolution" (p. 41).

Jack P. Greene's "The American Revolution: An Explanation" traces the break back to the imperial reforms begun in 1748. Some details are new, but the interpretation is the same "neo-Whig" view that Greene has ably presented for many years.

James Morton Smith's "John Adams and the Coming of the Revolution" is a very nice biographical sketch of the most outspoken of the Founding Fathers, mostly using Adams' own words. It begins and closes with President Kennedy's graceful remarks celebrating the publication of the *Adams Papers*.

Don Higginbotham's "James Iredell and the Origins of American Federalism" is the most original contribution in the book. He discusses the constitutional issues of the revolutionary and early national periods through the writings of an admittedly secondary figure who became associate justice of the U.S. Supreme Court. Iredell's views are interesting but hardly startling.

Overall, the book is worthwhile but not a "must." Cape Girardeau, Missouri had an impressive Bicentennial lecture series, but in print the lectures are too unstructured for general readers and their points of view too well known for scholars.

NEIL R. STOUT  
University of Vermont

LAWRENCE S. KAPLAN, editor. *The American Revolution and "A Candid World."* Kent, Ohio: Kent State University Press. 1977. Pp. xiv, 169. \$10.50.

This handsome, well-conceived little volume is the product of an American Revolution Bicentennial Conference held at Kent State University. There are three main segments: the first two essays deal with the role of George III in the dispute with the American colonies and with the three British at-



tempts at conciliation; the next two explore the intellectual origins of American foreign policy and its theory and practice; the third segment examines the Russian Czarina's perception of the Revolution and concludes with an analysis of the evolution of American ideas concerning the law of nations. Finally, an essay by the editor traces the history of the Franco-American alliance of 1778.

Of the seven essays, two—James H. Hutson's "Early American Diplomacy: A Reappraisal" and William C. Stinchcombe's "John Adams and the Model Treaty"—were originally delivered at a meeting of the Organization of American Historians in April 1976, while an earlier version of Lawrence S. Kaplan's "Toward Isolationism: The Rise and Fall of the Franco-American Alliance, 1775-1801" was presented in a colloquium at the Woodrow Wilson International Center for Scholars in July 1974. Hutson's essay has been previously published in the *Quarterly Journal of the Library of Congress* (July 1976), and in *Diplomatic History* (Winter 1977). Gregg L. Lint's "The Law of Nations and the American Revolution" also appeared in the same issue of the latter journal. The essays by Carl B. Cone ("George III—America's Unknown King"), Alan S. Brown ("The Impossible Dream: The North Ministry, The Structure of Politics, and Conciliation"), and David M. Griffiths ("Catherine the Great, the British Opposition, and the American Revolution") were written especially for the Kent State Conference.

Cone depicts a George III who was devoted to the British constitution, notably its tradition of parliamentary supremacy, and jealous of England's sovereign authority, and who "was involved with policy-making in no uniform or consistent way but variously, as circumstances differed, and rarely decisively" (p. 6). The struggle between the colonies and the mother country was "a great classic tragedy," because the dispute was insoluble in political terms acceptable to both sides. Analyzing the three British conciliation initiatives of 1775, 1776, and 1778, Brown reaches much the same conclusion, and agrees with Sir Lewis Namier that "the constitutional principles of the time rendered a conflict inevitable . . ." (p. 19).

George III, according to Griffiths, was a kind of prism through which Catherine viewed the American Revolution. Admiring the elder Pitt and other Opposition leaders, she despised England's monarch for incompetently throwing away an empire and worried about the future value of an Anglo-Russian alliance. She saw no larger significance in the American republican ideology.

The most striking aspect of the volume is the sustained attack on Felix Gilbert's influential thesis, set forth in his *To the Farewell Address: Ideas of Early American Foreign Policy* (1961), that Ameri-

can Revolutionary ideology contained an "idealistic internationalism," in addition to its other face of isolationism. Hutson offers a comprehensive critique of the Gilbert thesis, rejecting the claim that the French philosophes influenced American thinking, and concluding that "the ideal design of American Revolutionary leaders was not to use their foreign policy as an instrument to reform international politics, but to have no foreign policy at all, to move the country not toward idealistic internationalism but in the opposite direction, toward and indeed beyond isolationism to hermitry" (p. 49). Stinchcombe agrees, pointing out, however, that the isolationist ideal of the Model Treaty of 1776 had to be temporarily modified by the force of circumstance, i.e., the price of French cobelligerency. Lint's examination of evolving American views of international law, through which the United States sought to safeguard its neutrality in future, and Kaplan's treatment of the French alliance, in different ways support the isolationist thesis.

In sum, *The American Revolution and "A Candid World"* brings together a valuable and persuasive collection of pieces on the international diplomacy of the American Revolution and is a worthy addition to the literature of the Bicentennial.

ALBERT HALL BOWMAN  
University of Tennessee,  
Chattanooga

NEIL R. STOUT. *The Perfect Crisis: The Beginning of the Revolutionary War*. New York: New York University Press. 1976. Pp. xvii, 206. \$13.50.

The year 1774, highlighted by the Coercive Acts and the First Continental Congress, was a time of "perfect crisis" in the relations between Britain and her American colonies. The long decade of colonial protest was, in 1774, transformed into a larger imperial confrontation which would end in armed conflict. The explanation of that transformation from political to military resistance is the subject of this book by Neil R. Stout.

After a short background description of colonial America and the range of imperial difficulties prior to 1774, Stout begins the main part of his story with the passage of the Coercive Acts. "To the extent that Americans rebelled against the Coercive Acts," Stout concludes, "the war for American Independence was made in England" (p. 64). But the radical leaders of Boston, notably Sam and John Adams, also had a hand in starting the war, by instigating the Tea Party, driving Governor Thomas Hutchinson out of office, and leading the other colonies into a "massive united American response" to the Coercive Acts. The First Continental Congress became the means by which the

Boston radicals, whose political activities Stout describes at length, achieved a "united front against England." Americans in the fall of 1774 were in a defiant mood, and the Association and the Declaration of Rights and Grievances "were the product of American democracy" (p. 145). But a small minority of Americans did not agree with the outcome of the Philadelphia meeting, accusing the Boston radicals of a conspiracy to achieve independence from British rule. The Loyalists fulminated against both Congress and the Association in a "war of words" with the Whigs, but the effort was wasted. The tide of emotion ran against the Loyalists, who were forced to take refuge either in silence or in another land to escape Whig terrorism. The concluding section of Stout's book narrates the implementation of the Association, the rise of government by extralegal committees and conventions, and preparations for war. "The most revolutionary activity was, of course, in Massachusetts," Stout maintains (p. 165). The volume ends with the flow of events leading directly to Lexington and Concord.

Based on a limited selection of monographic literature, newspapers, and other primary sources, written with style and clarity, and amply illustrated, Stout's book is a quality narrative history of the dramatic events preceding the outbreak of war in 1775. It is a story well told, one which should thoroughly delight the general reading public.

ROGER J. CHAMPAGNE  
Illinois State University

R. ERNEST DUPUY *et al.* *The American Revolution: A Global War*. New York: David McKay Company. 1977. Pp. viii, 311. \$11.95.

Historians have long noted the international character of America's War for Independence. Indeed, many historians view the conflict as essentially a phase in the century-long Anglo-French competition for ascendancy. American scholars Samuel Flagg Bemis and Richard Van Alstyne have written diplomatic histories of the war from a multinational point of view, but until the appearance of *The American Revolution: A Global War* there has been no comparable attempt to study its military and naval aspects.

R. Ernest Dupuy, Gay Hammerman, and Grace P. Hayes open with a brief survey of the world balance of power in the decade following the Seven Years' War and outline the causes of America's revolt. Elsewhere they examine the role of foreign volunteers in the Continental Army and of non-Englishmen in the British Army and describe the peace settlement.

The heart of the work consists of an examination

of the interests of the French, Spanish, and Dutch in the struggle and an account of the wars they waged against Britain between 1778 and 1783. The lack of cooperation between Britain's enemies is clearly demonstrated, as is the fact that the entire conflict was to a great extent a maritime struggle in which the control of certain ports and areas spelled the difference between victory and defeat.

The book's strength is its descriptions of the Caribbean, European, Asian, and African theaters of war. These accounts serve as a corrective to the ethnocentric viewpoint of most Americans. Unfortunately the authors do not go beyond narration to assess the relationships between the events they describe. During the fall of 1781, for example, Admiral Graves faced superior French forces on the North American coast, Admiral Parker faced strong Spanish forces in the Caribbean, and Admiral Hardy faced superior French and Spanish forces in Europe. The events in each of these areas are recounted separately and never connected.

The authors do not account for American victory except by implication when they survey the forces arrayed against Britain. This can easily leave the false impression that Americans did not win the war but received independence as a by-product of Britain's worldwide struggle with her European enemies. Britain's attention was focused on the defense of Gibraltar and the English Channel during parts of 1779 and 1780, but during the rest of the period America remained Britain's chief concern, and the action there, especially in the Hudson River Valley and the Southern Campaign of 1780 and 1781, is what earned Americans their independence.

Similarly, the authors do not examine the strategies of the participants in any comprehensive manner. The role of American loyalists, for example, is largely ignored, even though British strategy and hopes for victory rested on them after 1778 when Britain's European enemies forced the deployment of portions of her military and naval forces elsewhere.

This book is not likely to attract the specialist, but its graphic narrative style and its vignettes of important figures make it valuable to the individual seeking an overview. The authors have drawn from a wide variety of secondary sources but the volume lacks documentation. They have produced an entertaining introduction to their topic which nicely supplements R. Don Higginbotham's analysis of the war from the American perspective in his *The War of American Independence* and Piers Mackesy's examination of it from the British perspective in his *The War for America*.

JAMES C. BRADFORD  
U.S. Naval Academy

DIRK HOERDER. *Crowd Action in Revolutionary Massachusetts, 1765-1780*. (Studies in Social Discontinuity.) New York: Academic Press. 1977. Pp. xvi, 394. \$19.50.

Dirk Hoerder puts forward in this volume the most comprehensive discussion of the role of the crowd in the American colonies yet attempted. Basing his study on extensive research in primary sources, Hoerder details the activities of crowds in Massachusetts Bay between 1765 and 1780, and proposes a rationale to explain their behavior.

Beginning with a lengthy discussion of historiographical concepts of crowd behavior, Hoerder turns to a consideration of crowds as an institution. He agrees with previous writers, such as Pauline Maier and Gordon Wood, that crowds were well disciplined and goal oriented. Hoerder notes that many of the objects of crowd action in colonial Massachusetts were imperial matters, such as impressment, but contends that such actions also reflected the beginning of class conflict within the society.

New England had once conformed rather closely to an ideology of corporate society in which the interests of the individual were subordinated to those of the community at large. Because of economic change, however, "the social primacy of the corporate system was challenged by the primacy of economic effect, . . . and equitable distribution and participation were sacrificed to liberalistic getting ahead" (p. 74). Riots, on the local level, were often aimed at displays of wealth or other symbols of the breakdown of corporate society. "Such attempts were never directed against the social or economic system but against individuals who manifested changes or who profited from them at the expense of the rest of the community" (p. 369).

During the Stamp Act crisis the established leadership found that traditional institutions had failed, and consequently turned to crowd action to obtain redress from specific grievances. This approach proved effective in resisting the Stamp Act, but the leadership then found its control threatened by the crowd. Hoerder contends that crowds, during this period, came primarily from the lower class, were highly politicized, and gave evidence of having developed a distinct ideology. Opposition to the Stamp Act "provided training for large sections of Bostonians and people in inland communities in direct and symbolic action against officials and wealthy persons, regardless of affiliation with the British or of colonial descent" (p. 143).

Threatened by the cohesive and coherent nature of crowd action, Massachusetts leaders adopted a "no mob" policy. This policy failed between 1768-70 when the continuing crisis over the Townshend

Duties again brought crowd action to the fore. Violence erupted in 1769, taking place on two levels: "Lower-class crowds began an active campaign against the troops and compliant leaders, while merchants directed a series of intimidating acts of violence against those merchants opposing nonimportation" (p. 242). Hoerder finds that divisions during this period "approached a kind of class alignment," but the deferential nature of the society permitted the leadership to direct most opposition toward other targets.

In part three Hoerder discusses the increasing militancy of the country towns, and the change, between 1770 and 1775, from direct action to a mass movement. He argues that the Boston leadership failed to understand the radicalism of the country and lost touch with what was going on. Country people, "less leadership influenced than Boston's population" had begun to develop a meaningful theory of "popular sovereignty."

In his concluding sections, Hoerder traces the role of crowd action after 1775. Having discovered that "officials and the wealthy, whatever their political persuasion, could be opposed was an irrevocable gain for the people" (p. 377). The people not only adopted the Whig theory of government, they applied it against their own leaders. In doing so, says Hoerder, they "transcended the limits of Whig thought, welding to it popular notions of equality and moving toward participatory and responsive political structures" (p. 381). Although the leadership groups in society managed to preserve much of the social status quo, "what Whig leaders had intended as a temporary diminution of deference and of submission to crown officials turned out to be a permanent change toward more critical supervision of authorities" (p. 383).

Hoerder's study is valuable more for its extensive and thorough discussion of crowd action in Massachusetts Bay than for the persuasiveness of his thesis. His emphasis on economic divisions and class conflict is an excellent corrective to previous accounts, but he sometimes strains credibility in his effort to make individual examples of crowd action fit into his overall conceptualization. He also makes some surprising errors of fact, as, for example, when he includes the Quebec Act as one of the Intolerable Acts and ignores the Quartering Act.

The editorial process in this volume is also faulty. Words are misspelled or transposed, the footnotes are difficult to use, and the index is too brief to be helpful.

Despite these criticisms, scholars will find this a useful volume. The research is exhaustive and much material is brought together which has not previously been available in one place. The per-

spective is fresh and thought provoking, and, despite some organizational problems, the account is generally well written and clear.

DAVID AMMERMAN  
Florida State University

RICHARD A. RUTYNA and PETER C. STEWART, editors. *Virginia in the American Revolution: A Collection of Essays*. Norfolk, Va.: Old Dominion University. 1977. Pp. viii, 92. \$2.50.

This volume is the first in a projected series designed as a vehicle through which new research can be presented about Virginia's role in the American Revolution.

Willard Frank's essay reviews the profusion of conflicting interpretations which stand as silent testimony to the elusive nature of the revolution. Frank finds it curious that motive has gone virtually unnoticed in the myriad investigations. Following this lead, he argues that the eighteenth century is filled with guideposts to explain elements in Colonial society that resulted in "dis-equilibrium." The revolutionary generation was buffeted by a series of dramatic changes: the Great Awakening, the socioeconomic decline of New England, the imperatives of slave culture, and the influx of a different type of immigrant. As a consequence, Americans lacked an identity. The imperial crisis provided Americans with a common framework and an opportunity for a great "psychological release of tensions." Although the evidence is not yet definitive, Frank's encouraging line of enquiry promises to bind together many of those disparate pieces of the Revolution.

The remaining essays are more narrowly conceived. William Parks focuses upon religion as a precipitant to revolution. Although Alan Heimert's seminal work concluded that conservative evangelical religion provided eighteenth-century America with a liberal democratic political ideology which led to separation from Great Britain, Parks notes that it was based primarily upon the New England experience. Would it be equally valid if applied to Virginia? Parks reviews the development and proliferation of dissenting denominations in the colony. He concludes that although the same religious divisions are found in Virginia as New England, in the former both religious groups (dissenting and Anglican) adopted attitudes toward the approaching revolutionary crisis which were complementary. Peter Stewart looks at the economic results of the Revolution. Was the Confederation a period filled with economic adversity, the conclusion drawn by many historians, or was it an era of strength, as argued by Merrill Jensen? Stewart suggests that an investigation of Norfolk and the Elizabeth River commerce before

and after the Revolution might contribute a greater understanding to the question. Carefully compiling statistics for both periods, Stewart argues that, with the exception of greater commercial diversity, Norfolk's economic life was not significantly altered by the war. The Virginia economy during the Confederation is also the subject of Robert Bittner's essay. Pointing to the resentment of local planters and merchants over the continued dominance of British traders after 1783, he, too, concludes the Revolution resulted in little initial change in Virginia. To overcome the problem of monopolization and other difficulties incident to independence, the Virginia legislature enacted the Port Bill of 1784, conceived and sponsored by James Madison. Although designed as a panacea for all of Virginia's economic problems, Bittner concludes that the bill failed because of anachronistic goals. The city market was a seventeenth-century ideal and particularly curious in the context of the agrarian-oriented ideology of Jefferson and his followers in the next decade.

Although the essays are of uneven quality, this thin volume makes a real contribution to the literature of the period. It deserves to be read by both scholar and general reader.

RICHARD M. JELLISON  
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Ohio

J. BARTON STARR. *Tories, Dons, and Rebels: The American Revolution in British West Florida*. Gainesville: University Presses of Florida. 1976. Pp. ix, 278. \$8.50.

Thorough, competent, and workmanlike are adjectives that may properly be used in assessment of J. Barton Starr's study. The stage is effectively set, the chapters reflect a natural subdivision of the larger topic, and the subject is fully covered. The documentation is impressive both for its inclusion of virtually all relevant printed material and for the wealth of primary sources brought to bear.

As the bibliography and Starr suggest, the subject of West Florida and the American Revolution is not a new one. However, Starr adds a significant amount of flesh to the body of knowledge previously available. He does so through extensive use of such sources as Colonial Office Papers, where the volume of applicable documentation is almost forbidding. Citing J. Leitch Wright, Jr., *Florida in the American Revolution*, James A. James, *Oliver Pollock*, and John Walton Caughey, *Bernardo de Gálvez in Louisiana*, Starr points out that there is no study of the subject from the British point of view. This he provides, in some cases in perhaps unwarranted detail.

The ten substantive chapters, cleverly titled, are



arranged chronologically, but each has a discrete focus. Starr's description of the problems of the establishment of government after 1763 and the early impact of the American Revolution provides good basic information for the more critical period that followed. Willing's raid receives careful attention, reaffirming the conclusion that its effect was principally detrimental to the revolutionary cause among the Floridians. Subsequent chapters focus on the efforts of British officials to strengthen the military posture of the colony in the face of Spanish attacks out of New Orleans, and the several phases of the conquest of West Florida by Governor Bernardo de Gálvez. Finally, Starr analyzes the larger reasons for Spanish success and then concerns himself with the fate of British loyalists in West Florida after 1783.

His conclusion is that West Florida had little connection with the American Revolution because of isolation, the influence of royalist refugees, the lack of meaningful grievances against the mother country, the relatively large number of troops present in the colony, and the compelling factor of the war with Spain. He finds the most significant outcome of the entire course of events to have been the reacquisition of Florida by Spain, a nation too weak to resist subsequent American expansion.

The principal weakness of the book is its style. The writing is somewhat stiff, formalistic, and unnecessarily wordy. Redundancy is not uncommon. Quotations are used much too frequently; especially brief excerpts which serve none of the functions which quotations normally provide. And one splendidly provocative sentence slipped through all the readings: "Following his death on March 21, 1779, Chester appointed a five-man commission to run the office of superintendent . . ." (p. 137).

For fairness of emphasis, however, allow me to repeat the original assessment. The study is thorough, competent, and workmanlike.

BERNARD E. BOBB  
*Washington State University*

JOHN CHESTER MILLER. *The Wolf by the Ears: Thomas Jefferson and Slavery*. New York: The Free Press. 1977. Pp. xii, 319. \$12.95.

Here is neither a stereotyped Jefferson nor a quasi-fictional creation. Here we meet the man of Monticello and the thinking of that man as the evidence shows them to have been. John Chester Miller presents a discussion of Jefferson, slavery, and the Declaration of Independence, chapters on "Slavery and the Rights of Man," "Slavery and the Revolution in Virginia," and "Slavery and the Ordinance of 1784"—with Jefferson's name prominent in the discussions although absent from the titles. The latter generalization likewise applies to

most of the next fourteen chapters, from "Slavery and the Decline of 'Republican Virtue'" through "The Abolition of the Slave Trade."

Slightly beyond the halfway mark, we find a model summary of "Jefferson and James Callender: The Politics of Character Assassination." This is followed by "The Sally Hemings Story"; "Jefferson and Maria Cosway"; "Jefferson, Mrs. Walker, and the Freedom of the Press," and "Jefferson, John Marshall, and Slavery." The last fifty-nine pages guide the reader from "The Missouri Controversy" through "The Last Word from Monticello."

Among the points excellently developed are Jefferson's hatred of slavery; what he did—and what he did not do—to oppose the institution; the relationships between ideas or ideals and action or negation in the Jefferson record, and his view of blacks as temporary residents on American soil. Removing blacks from the United States was clearly what Jefferson desired. But he "quailed" before the prospect of spending nearly a billion dollars for compensation, transportation, and resettlement.

"We have the wolf by the ears," Jefferson declared in 1820, "and we can neither hold him, nor safely let him go. Justice is in one scale, and self-preservation in the other." In 1824, Jefferson "was prepared . . . to make his immediate objective the removal of black slave children." "The abolition of the evil [slavery] is not impossible," the octogenarian said less than a year before his death. "It ought never therefore to be despaired of. Every plan should be adopted, every experiment tried, which may do something towards the ultimate object." Not only near the end but throughout the volume, Miller attempts to answer each pertinent "why" in addition to every "what" and "how". Thus things which Jefferson did or said are reported in context.

The book has a few typographical errors. The notes (in the back, and somewhat abbreviated) might have been improved on. And one wonders whether such offerings as "Price Hall" (p. 75) for Prince Hall and "Pee Wee" (p. 173) for Pee Pee (Ohio community of Madison Hemings) are indeed to be blamed on typography. In the preface, Miller describes his analysis as "necessarily episodic"; he may have been too self-critical, for "topical with chronological overtones" seems fairer. Still, no matter how one summarizes particulars of the Miller technique, emphasis should be placed upon its success. Balanced presentations of facts repeatedly are accompanied by discerning interpretations, accurately reflecting the author's judicious mind.

HOLMAN HAMILTON  
*University of Kentucky*



DALL W. FORSYTHE. *Taxation and Political Change in the Young Nation, 1781-1833*. New York: Columbia University Press. 1977. Pp. x, 167. \$15.00.

This study derives from a prize-winning doctoral dissertation in political science, and its purpose is to determine the validity of propositions advanced by political scientists to explain American politics. Dall Forsythe chooses taxation as his subject because he considers it the paramount issue in the period 1781 to 1833, as the federal government, moved by the internal dynamics of political power, endeavored to expand its "extractive capacity" and thereby enlarge the scope of its functions and authority. Forsythe initiates an inductive process by first laying out his data in the form of a brief history of taxation from late Colonial times to the War of 1812 and on through the battle over protective tariffs culminating in the Nullification Controversy of Jackson's administration. His history is based on the contents and interpretations of four or five good historical studies of each of the several periods he contemplates. He does an excellent job of synthesis, and although his unfamiliarity with source material surfaces occasionally, his summary of taxation is intelligent and perceptive.

Except for specialized terms such as "regime," "zones of indifference," and "authority crisis," the first four chapters read like history. The last two are unmistakably political science, and in them Forsythe tries to formulate some analytical principle about American politics that is consistent with the events of the period he has covered and possibly even true in general. He finds that various hypotheses advanced by political scientists do not fit the facts, and so he produces an "alternative typology" of his own, dividing American politics into four categories: normal politics, regime politics, and two kinds of crisis politics—a formula scarcely Einsteinian in simplicity.

Although by the time its terms are defined and refined such a formula may well appear as inexact as common language, the attempt at definition is by no means without suggestive value. It exhibits the attempt of political scientists to invoke scientific method, and even if a little Cartesian in approach, it is nevertheless instructive.

E. JAMES FERGUSON  
Queens College,  
City University of New York

HOWARD JONES. *To the Webster-Ashburton Treaty: A Study in Anglo-American Relations, 1783-1843*. Chapel Hill: University of North Carolina Press. 1977. Pp. xx, 251. \$15.95.

Anglo-American relations drifted from one petty crisis to another after 1837. At issue were twelve

thousand square miles of marginal soil along the Maine frontier—a legacy of faulty maps in 1783—and a broad spectrum of less tangible conflicts: American sympathy for Canadian independence movements, British disgust at America's lackadaisical enforcement of its own laws against slave-trading, conflicting interpretations of maritime law, and others. For neither nation were vital interests involved, as Howard Jones repeatedly makes clear in this well-researched volume. What elevated these irritating but essentially minor problems into political and diplomatic crises was emotionalism: the centrality of honor, prestige, and mutual distrust. Successive presidents and prime ministers could neither resolve nor ignore these emotions, and the result was a consistent tendency on both sides of the Atlantic to elevate every disagreement into an excuse for a third Anglo-American war.

It is clear that Jones considers Daniel Webster a great secretary of state. After preliminary chapters revealing new details about the *Caroline* affair, the comic-opera Aroostook War, the trial of Alexander McLeod, and the *Creole* slave ship case, Jones devotes nearly a hundred pages to a careful reinterpretation of Webster's negotiations with British Special Minister Lord Ashburton. In the process, Jones acquits Webster of every major charge leveled against him during the past century, and there have been many. The most sensational accusation—that Webster took money from Ashburton to expedite a settlement—sprung from the writings of Samuel Flagg Bemis, and Jones is particularly thorough in his attempts to prove Webster innocent. Those familiar with the "red line maps" controversy will welcome Jones' detailed and plausible analysis of why those maps were irrelevant to the negotiations. And while both passages will confuse many readers, the fault lies less with Jones' writing, which is adequate, than with the bewildering maze of evidence he has examined and must relate.

There are weaknesses, many of which careful editors would have caught. The book is misnamed, for despite the promise in his title, Jones devotes only sixteen pages to the years 1783-1837, and these constitute the weakest section of the book. While Jones correctly emphasizes the degree to which the final settlement was a hostage to local interests, he often confuses that important idea with states' rights. Furthermore, there are gratuitous and sometimes inexplicable judgments: that Webster's barrage of literature in Maine newspapers favorable to a settlement was somehow not propaganda (p. 94); that the Maine boundary question, seen as "petty" by British cabinets (p. 36) and viewed with "indifference approaching boredom" by one American president (p. 41), was

a "grave interest" to both governments (p. 180); and, in a particularly questionable judgment, that "the most far-reaching consequences of the agreement were military" (p. 136).

Despite such lapses, however, this is a solid book. Jones has examined every important source in Britain, Canada, and the United States, and his work represents the fullest account of the negotiations ever written. It should not soon be superseded.

DONALD S. SPENCER  
*University of Montana*

JEROME R. GARITEE. *The Republic's Private Navy: The American Privateering Business as Practiced by Baltimore during the War of 1812*. (American Maritime Library, number 8.) Middletown, Conn.: Wesleyan University Press, for Mystic Seaport, Inc. 1977. Pp. xx, 356. \$17.50.

Privateering has been described in exciting narratives of skill and adventure by George Coggeshall and Edgar Maclay. Now Jerome R. Garitee abandons the traditional "yo heave ho" approach to present a detailed analysis of the business side of privateering in Baltimore during the War of 1812. By narrowing his scope, Garitee is able to reveal the practical support system for a much romanticized enterprise.

In the best part of his book, Garitee examines the men who invested in privateers. Today, historians stress the national, religious, and family ties that bound merchants into associations. But the only entry requirement among Baltimore's privateering merchants was money. After all, it was an expensive business. With the \$10,000 cost of a share in a first-class privateer, a man could start his own business. Half of all investors lost money, as only one in three prizes reached a safe port; however, the more risked the more gained. Active investors who entered privateering as soon as the war began did better than marginal or moderate investors. The caution of smaller investors encouraged them to delay action until after the British had blocked Chesapeake Bay.

Garitee also provides a panorama of the people who profited from privateering. One-fifth of Baltimore's working men, from merchants to maritime tradesmen to sailors, depended upon privateering for their livelihood. Even the United States government became an unexpected beneficiary when it placed exorbitant duties upon captured cargoes after several rich prizes entered port.

Although Garitee tries to give special attention to the armed traders that left Baltimore for specific ports (in contrast to privateers that cruised about for prizes), he does not provide new information about them. As they outnumbered privateers three

to one, and were supposed to attract the best sailors, armed traders will undoubtedly become the subject of future studies.

As the title suggests, Garitee seeks to portray Baltimore privateers as a kind of private navy upholding the honor of the United States while the small national navy was blockaded by British warships. This interpretation touches on a common dispute among maritime historians. It has been argued convincingly that, while privateers did disrupt British commerce, they did not alter the course of the war. Indeed, American privateers with their promise of prize money made it very difficult for the American navy to man its warships. Whatever the reader's interpretation, he will find this work of value as maritime, business or urban history. It is not a startling piece of research, but it marks a solid improvement on what has gone before.

JOSEPH A. GOLDENBERG  
*Virginia State College*

JOHN S. MORGAN. *Robert Fulton*. New York: Mason/Charter. 1977. Pp. xi, 235. \$10.95.

After one gets past the startlingly pretentious prologue, which declares that Robert Fulton was the "inventor of the modern concept of invention," "one of the world's first technologists," "one of the world's first military technologists," and "one of America's first engineers," it is a relief to find that what follows in this book by John S. Morgan is a sound and readable popular biography. The author follows Fulton through his unsuccessful artistic career in England and his equally unsuccessful career in France as a would-be military engineer, giving us a picture of a man from a poor background who wanted desperately to succeed and to gain recognition in the world. His ideas for canal improvements and submarine warfare were impractical, and Fulton survived by the generosity of friends, especially the Joel Barlows, by virtue of his commanding personality and his ability to convince influential people, at least temporarily, of the brilliance of his schemes. Though Morgan is hesitant to brand Fulton a charlatan, the facts presented paint him as at least a moral chameleon, especially in his attempts to sell his submarine warfare ideas to France, Britain, and then the United States. But he was no worse in this respect than a typical Renaissance engineer.

His opportunism becomes even more evident when he returned to the United States to put together the right combination of elements to produce a commercially feasible steamboat. Hitherto deeply opposed to any aspect of monopoly privilege, Fulton devoted his life and what fortune he

had by that time to trying to establish monopoly privileges for his boats, first on eastern, then on western rivers. Had he concentrated on simply building his competent boats and running them, he would have died a rich man. But his pride and his disinclination to be known as a "boat builder" led him into a series of disastrous lawsuits, which he seems to have enjoyed even though they brought him ruin.

Morgan concludes, "as an inventor, Fulton was largely a compiler, adapter, and sometimes improver of others' discoveries" (p. 205)—hardly compatible with his prologue. A quote from English engineer John Rennie perhaps sums it up even better, though harshly, "I consider Fulton, with whom I was personally acquainted, a man of very slender abilities though possessing much self confidence and consummate impudence" (p. 206).

Flaws in the book include much supposing that Fulton met various people in his early career, sometimes erratic paragraphing, and a tendency toward occasional gratuitous remarks (such as noting that there was no hint of a homosexual relationship between Joel Barlow and Fulton—if so, why mention it). The facts presented give a sound picture of the man and his environment, the technical discussion is good, and the book is a useful piece of work.

MONTE A. CALVERT  
Clarkson College of Technology

KEITH E. MELDER. *Beginnings of Sisterhood: The American Woman's Rights Movement, 1800-1850*. (Studies in the Life of Women.) New York: Schocken Books. 1977. Pp. 199. \$11.95.

Keith E. Melder's 1963 doctoral dissertation, "The Beginnings of the Women's Rights Movement in the U.S., 1800-1840" was such a fresh and valuable contribution to understanding women's history at the time that it was approvingly cited in path-breaking works by Aileen S. Kraditor and Gerda Lerner. After fourteen years of concentrated scholarship in women's history, however, Melder's book on the same subject appears as a succinct summary of existing knowledge rather than a pioneering effort. The most illuminating points in Melder's dissertation were the links he drew between women's early participation in religious, charitable, and social reform efforts, the growth of their sense of individual autonomy and collective sisterhood, and the appearance of an organized women's rights movement. Scholars in the interim have developed these points with more detail, complexity, and sophistication than Melder can supply in this short book.

In *The Beginnings of Sisterhood* Melder provides

introductory chapters on the early nineteenth-century ideology of "women's sphere" and on progress in women's education. He then outlines six "distinct types" of sisterhood accessible to women in the period: two traditional types, among kin and individual friends; and four new types, arising from educational institutions, religious societies, benevolent efforts, and industrial work. After setting the background of women's efforts in moral reform, peace, temperance, and schoolteaching, he concentrates on women's activities within the abolition movement, the immediate matrix of the women's rights movement. Subsequent chapters reveal the Grimké sisters' dramatic contributions and the furor over "the woman question" within the abolition movement. Discussing other leading lecturers and contributors to the formation of a movement for women's rights, on the one hand, and conservative opposition from within the camp of vocal women, on the other, Melder brings the narrative up through the 1848 Seneca Falls convention. Along the way he provides thumbnail biographies of many women besides the Grimkés—Abby Kelley Foster, Lucy Stone, Lucretia Mott, Margaret Fuller, Harriot Hunt, for example—and quotes liberally, and to good effect, from the letters and personal documents of these women and others.

At the conclusion of every chapter Melder pauses briefly to interpret or assess the significance of the information offered, or to suggest questions still to be pursued. These are the most engaging parts of the book; nevertheless, the volume as a whole has the repetitive rhythm of narrative and review, narrative and review, rather than sustaining an interpretive dynamic. It is founded on original research, but its style suggests a work so revised, augmented by other scholars' analyses (although only a fraction of the relevant current work is cited), pruned, and reprinted, that the original intent of discovery that vivified the findings has been lost sight of.

Melder's concluding interpretation of the origins of the women's rights movement follows, in part, the "status-deprivation" argument offered by Gerda Lerner in "The Lady and the Mill-Girl" (*Mid-Continent American Studies Journal*, 10 [1969]). In his first chapter, Melder implies that middle-class women experienced not only a relative but an actual decline in status in the late eighteenth and early nineteenth century. But his positive assessment of women's reform activities, especially in abolition, preceding an organized women's rights movement, powerfully suggests the opposite: that the nineteenth century, even early on, was "the century of the woman" as none had been before. The other reasons Melder brings to his explanation of the origins of the women's rights movement—women's shared participation in kin-

dred reforms and in religious conversions and crises, the sense of sisterhood derived from education and religion and reform, and the geographical overlap between districts of evangelical fervor and of women's rights organizing—are wholesome and useful but not conclusive. These may describe the necessary but cannot describe the sufficient conditions for a women's rights movement to develop, since they apply as well to plenty of nineteenth-century women who never initiated nor were drawn into protestations for women's rights.

NANCY F. COTT  
Yale University

THEODORE DWIGHT BOZEMAN. *Protestants in an Age of Science: The Baconian Ideal and Antebellum American Religious Thought*. Chapel Hill: University of North Carolina Press. 1977. Pp. xv, 243. \$14.95.

Scottish common sense realism as a kind of Protestant scholasticism has engaged the attention of American intellectual historians for at least two decades. The emphasis has been upon moral philosophy, especially as it bore upon matters of practical ethics in economic, political, and social life. Theodore Bozeman directs his research into an equally significant area, the combined efforts of mental, moral, and natural philosophers to meet the challenge of pre-Darwinian science.

The origins of this united front lay in the ambivalence of more sophisticated evangelical Protestants toward the Enlightenment heritage: on the one hand, they applauded its demonstrably progressive features, while on the other, they feared the very skepticism which produced these results. Much of the difficulty seemed to stem from the opening Lockean epistemology had afforded to speculation divorced from induction, which, in turn, contributed to a proliferation of allegedly baseless theories, as much at odds with scientific as with theological fact. Within this context, the rigorous empiricism of Francis Bacon seemed altogether preferable to the troublesome sensationalism of Locke. Moreover, upon the broad ground of the Baconian empirical ideal, an impressive group of religiously committed scientists and scientifically aware theologians could join forces to create what the author calls a "doxological science" that was both intellectually satisfying and spiritually reassuring.

The essence of this position was the belief that an inductive approach, free from speculative distortion, to the phenomena of modern science would yield indubitable evidence of the moral government of the cosmos. Notwithstanding disturbing countercurrents, doxological science held the ascendancy in educated circles by mid-century. Its

expressions were varied, but perhaps the best single example of both the optimism and the precariousness of this faith was the identification of geological catastrophism with theological millennialism. With such weapons, evangelical intellectuals armed themselves against both materialism and transcendentalism.

Bozeman focuses throughout on the Old School Presbyterians. This body was evangelical in doctrine, but its intellectualism, expressed in respect for natural science and systematic theology, distinguished its spokesmen from the more enthusiastic Methodists and Baptists, whom they criticized for mindless emotionalism. While this stress upon conservative Presbyterianism is justifiable, one feels sure that a thought pattern so timely must have found considerable acceptance among other major denominations as well.

This is an intricately reasoned monograph in the history of ideas which rightly makes heavy demands on the reader. The author, however, might have eased the burden, had he fixed more clearly on a central theme and held to it. As it is, Old School Presbyterianism, doxological science, and the Baconian ideal seem caught in an unresolved contest over which should bear the weight of the argument. Bozeman makes his case for the importance of Baconianism, but it might have served him better to treat this as a subordinate ingredient of doxological science. Despite such structural problems, this is a valuable addition to the history of religious and scientific thought in the United States before the Civil War.

RAYMOND J. CUNNINGHAM  
Fordham University

CHARLES CAPEN MCLAUGHLIN and CHARLES E. BEVERIDGE, editors. *The Papers of Frederick Law Olmsted*. Volume 1, *The Formative Years, 1822 to 1852*. Baltimore: Johns Hopkins University Press. 1977. Pp. xx, 423. \$20.00.

Charles Capen McLaughlin has been working on Olmsted for over twenty-five years now, and he brings an authoritative expertise to this edition of Olmsted's papers. He and Charles E. Beveridge have selected "the best" of their subject's letters and other writings for publication in this volume and the seven which are to follow, all arranged chronologically save for a volume devoted to Olmsted's works on landscape design. This first volume has been handsomely produced, and the care with which the editors have undertaken their task is evident on every page.

The volume opens with a fifty-page biography of Olmsted by McLaughlin, which is informative but alas a little dull. The editors also provide a useful



biographical directory offering short sketches of the people who inhabited Olmsted's world, several well-chosen illustrations, and some instructive genealogical charts. Flanked by this scholarly scaffolding are 290 pages of Olmsted himself, consisting chiefly of some autobiographical fragments and his letters to his relatives and friends. There are no great surprises in what Olmsted has to reveal, but his youthful curiosity, combined with his powers of observation and expression, make for interesting reading.

These are the letters of a young man in his twenties, a man still seeking to find out about himself, the world, and his own place within it. At the conclusion of this volume Olmsted still has not visited the South or begun his career as a landscape architect, and historians of those subjects will have to await future volumes. The material in the present volume will not aid any particular type of historian especially—save perhaps biographers of Olmsted or historians of youth—but some may find instructive Olmsted's advocacy of scientific farming, some his antisectarian theology, some his tempestuous voyage to China, some his rather patrician comments on politics. A strength of the volume is the amount of research which sustains it. The editors append between ten and thirty informative footnotes to each letter, identifying as far as possible all the incidents and persons referred to and explaining colloquial expressions and other unfamiliar phrases. They have shown themselves more than capable of coping with the harassing demands of editorship.

But the chief delights are the occasions when Olmsted vividly brings his world to life. Thus we learn of a small Connecticut community of the 1820s which contained a single dissolute family, who were dutifully prayed over as "the poor." There is Olmsted's intriguing joke that Yankee turnpike operators deliberately measured out short miles in order to increase their tolls. For those of us who like to read significance into 4th of July orations, there is the cautionary tale of one such occasion when an orator known for extravagant hyperbole was conned into speaking, so that the townsfolk could have a good laugh at his expense. And there is the village of Guildford, Conn., where the Whigs and the Abolitionists each had their own physicians, while the Democrats were enthusiastic herbalists. Perhaps the time has come for a medical interpretation of voting behavior!

M. J. HEALE  
University of Lancaster

GEORGE WALTON. *Fearless and Free: The Seminole Indian War, 1835-1842*. Indianapolis: Bobbs-Merrill Company. 1977. Pp. 274. \$13.95.

At first glance, *Fearless and Free* promises a fresh assessment of the 1835-42 conflict with the Seminole Indians in Florida. Unfortunately, it becomes only a pallid imitation of John Mahon's definitive treatment of this topic in *The Second Seminole War* (1967). Military, Indian, and Florida historians will look in vain for new treatments of data or revisionist interpretation of the causes, conduct, and aftermath of the war. In fact, the work is overly dependent on standard secondary sources, particularly the Mahon treatise.

The narrative is a chronological presentation of events in Florida from Spain's cession of the territory to the United States in 1819 through the conclusion of hostilities in 1842. The Seminoles, under constant pressure from the territorial legislature for relocation, were to be another victim of the Jackson administration's removal policy in the 1830s. But unlike other eastern tribes, the Seminoles presented unique problems because of their geographical isolation, a sparse frontier population, and their fierce opposition to removal. After the failure of initial attempts to resettle the tribe on a reservation in Florida, the government secured a spurious treaty authorizing their emigration to the Indian Territory. The antiremoval faction led by Osceola and the Seminole Negroes resisted and the government resorted to force. The Second Seminole War dragged on for seven years, costing the federal government over forty million dollars, some 1,400 troops, and countless settler and Seminole lives, and the nation had its most unpopular war until the Viet Nam conflict.

The book contains some interesting lithographic illustrations, although it suffers from a lack of maps on which to follow the widely scattered engagements of what was essentially a guerrilla war. There are some disconcerting inconsistencies in spelling; for example, Lt. Bassinger is spelled Bessenger, yet a fort named in his honor is called Fort Bessinger. In another place the town of Mandarin is mislocated on the Indian rather than the St. Johns River. There are similar editorial curiosities throughout.

In fairness to Walton, this book was doubtless intended for popular readership rather than for historians of the period, and as such probably should not be judged too harshly. At least one major historical quarterly is reviewing it as a youth book. It could be interesting fare for the casual reader.

HARRY A. KERSEY, JR.  
Florida Atlantic University

RUDOLPH M. LAPP. *Blacks in Gold Rush California*. (Yale Western Americana Series, number 29.) New Haven: Yale University Press. 1977. Pp. xiv, 321. \$15.00.



Ever since Rudolph M. Lapp's article, "The Negro in Gold Rush California," appeared in the *Journal of Negro History* in 1964 persons interested in the neglected theme of the Negro in the Far West have been looking forward to a book-length study. The present volume—thoroughly researched, intelligently organized, and effectively presented—is well worth the wait.

Although the experiences of western Negroes varied widely from state to state, their California history was in various respects unique. The Afro-American migration to California was the earliest major movement of American Negroes neither involuntarily accompanying their masters nor fleeing slavery but primarily—as free men—seeking greater economic opportunities. Although, of the approximately five thousand black migrants, some were slaves, the overwhelming majority had previously known freedom and many even a degree of prosperity. The ocean-borne migrants had sometimes previously organized into companies. Negroes from the whaling and abolitionist town of New Bedford were conspicuous, notably the self-educated Scottish-Gayhead Indian-mulatto Jeremiah Burke Sanderson, who pioneered in both education and religion. These blacks of the Gold Rush decade "represented a higher degree of initiative, aggressiveness, and tenacity than most Americans, black or white."

Certainly they needed these qualities, for although they often enjoyed considerable success, sometimes even as members of integrated companies, the attitudes and institutions of the anti-Negro border states soon became dominant, and proslavery Southerners in 1852 even put through a California fugitive slave law. The California Negroes, many of whom had been active in the abolition movement, fought back vigorously. In the famous Archie Lee case of 1858 they and their white allies won a conclusive victory. Against such discriminations as exclusion from the public schools and from testifying in cases involving whites, they employed a series of colored conventions. But despite support from hundreds of whites, the convention movement ultimately failed. This failure, and an anti-Negro immigration measure which missed passage only through a technicality, led in 1858 to a large-scale exodus to British Columbia. The majority who remained behind had to wait for the Civil War and the fourteenth amendment before achieving the elementary rights so long denied them.

Although the emphasis is on the mother-lode country and San Francisco during the Gold Rush decade, a brief chapter treats southern California and another is devoted to the few Afro-Americans in California prior to 1849, of whom by far the most important was the wealthy landowner and busi-

nessman William A. Leidesdorff, a Danish mulatto from the West Indies "whose contemporaries considered him white."

Readers of this study will now look forward to a volume bringing the story of the Negro in California closer to the present date.

KENNETH WIGGINS PORTER  
*University of Oregon*

DAVID R. GOLDFIELD. *Urban Growth in the Age of Sectionalism: Virginia, 1847-1861*. Baton Rouge: Louisiana State University Press. 1977. Pp. xxix, 335. \$17.50.

In a thoughtful essay ten years ago, Dwight Hoover suggested that studies in American urban history followed two "diverging paths," the first being a route taken by scholars who wished to establish a role for cities in our national history, the second a course taken by social scientists who wished to illuminate the process of urbanization. David R. Goldfield's most recent study in this field is a skillful exercise in merging the two major divisions. In his Virginia case study, Goldfield explores the relationship between the burst of city expansion during the fifteen years before the Civil War and the sectionalism which ultimately led to armed conflict. His major thesis is that a common concern for urban economic development in the mid-century decades helped to create sections which had much more in common than the rhetoric of the times would indicate.

In his examination of urban growth, Goldfield details the railroad and canal promotional campaigns that helped to establish an intrastate transportation network which linked Virginia cities with trade centers in other regions of the country. Within the framework of an emerging state and national urban network, there is an extended analysis of such indicators of city maturation as the expansion of commerce and manufacturing, the increase of city services, and the development of an urban consciousness. Making all of this occur are the movers, shakers, and boosters, who are subjected to detailed scrutiny by Goldfield in his role as social scientist. The portrait of "city builders," which is presented in the central chapter of the study, consists of aggregated characteristics of the urban leaders in the state's leading cities; while in Richmond a statistical test of significance was used to identify the features which would most likely distinguish its boosters from their fellow citizens.

The burst of economic development in the decade and a half before the Civil War had ironic results for the elite who facilitated urban growth: rather than achieving independence, they were

tied into a national economy and network of cities dominated by an urbanized North which possessed a superior transportation system that funneled raw materials and manufactured products through its ports. In this context, Goldfield demonstrates that urban Virginians faced a profound dilemma after the election of Lincoln: their continued advancement depended on their ties to the North, at the same time that their position in the Union was one of economic and political subservience.

Although the book ends with secession, an epilogue discusses the events of the previous fifteen years within the frame of reference of other works which contrast the Old and the New South. To Goldfield, the boosterism of the New South was a continuation of a process and style which could be seen in the Old Dominion before the war. While this interpretation is not new, it does offer weight to the argument that the prewar South was not an undeveloped, agrarian society.

This thoroughly researched study will be of interest both to urban historians and to students of southern history whom Goldfield urges "to place the magnolias on Main Street and to develop an urban view" of their field (p. 283). The lucid, graceful style almost absorbs the plethora of tables: in only one place—the crosstabulation tables—might the text have been lightened and the appendix lengthened.

TIMOTHY J. CRIMMINS  
Georgia State University

EDWARD G. LONGACRE. *The Man Behind the Guns: A Biography of General Henry Jackson Hunt, Chief of Artillery, Army of the Potomac*. Cranbury, N.J.: A. S. Barnes. 1977. Pp. 294. \$8.95.

This is the first biography of one of the foremost Union artillery officers of the Civil War. For a century after the war these officers have been thought of as unimportant "behind the line administrators." Obviously this carefully researched and well-written biography fills a long-standing vacuum in the field of Civil War history. Edward G. Longacre has published two earlier books on the Civil War and is well acquainted with the range of material on that era of American history.

Orphaned early in life, Henry J. Hunt was reared by relatives and educated at West Point, where he attained an average academic record and a better than average demerit standing. His strongly held opinions, often uttered thoughtlessly, proved a liability throughout his active military career and during his retirement.

Hunt served in several frontier posts, wrote *Instructions for Field Artillery* (later used by both sides

in the Civil War), and gained valuable military service in the war with Mexico.

In 1861 he became a major in the Fifth U.S. Artillery and joined General McDowell's army in northeast Virginia. Despite McDowell's defeat Hunt was made Chief of Artillery of the Department of Northeast Virginia. The next year he became head of Artillery Reserve and later served as a colonel on General McClellan's staff. By the age of forty-three Hunt was a brigadier general, assigned to General Burnside's Command. Burnside was replaced by Hooker who, in turn, was succeeded by Meade. Hunt's career reflected the frustration resulting from the frequent shifts in command. His unit won laurels at Missionary Ridge in the battle of Gettysburg, but General Meade was dissatisfied with Hunt's artillery and forced him to request a transfer.

Hunt began the 1864 campaign in charge of General Grant's artillery; Grant subsequently dissolved Hunt's unit and sent some of its members to defend Washington. Hunt, in 1865, was made Major General of Volunteers and Regular Services. At the end of the war Hunt's Artillery was probably one of the best arms of the nation's military service.

From 1865 until 1883 Hunt briefly served in several Southern states including a stint as head of the Department of the South. He retired in 1883 after forty-nine years of service with his health and finances deteriorating seriously. Efforts to have Congress increase his pension failed. He lived out his last years as governor of the Washington Soldiers' Home and died in 1889 in poverty.

GEORGE OSBORN  
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JOEL H. SILBEY. *A Respectable Minority: The Democratic Party in the Civil War Era, 1860-1868*. (Norton Essays in American History.) New York: W. W. Norton. 1977. Pp. xviii, 267. \$10.95.

The Northern Democrats of the 1860s have presented an uninviting face to political historians. Out of power, split by warring sections, beset by factionalism, the Civil War Democrats have often appeared as a lackluster remnant opposed to the war. The historian Roy F. Nichols brought the Democracy through the disruption of 1860, and until the present work by Joel H. Silbey no one has attempted a general analysis of the troubled party during the next eight years.

The lack of constructive leadership and measures as well as of valuable manuscript sources goes far to explain historians' neglect. After the death of Stephen A. Douglas party leadership fell to lesser political figures like C. L. Vallandigham,

Horatio Seymour, and George B. McClellan. The Democrats asserted they were for the Union, but their outlook was antiabolitionist, virulently racist, particularist in constitutional principle, and pacific toward an enemy warring for independence. Silbey does not address the nagging question of whether the Democrats could have won the war through their measures.

A practitioner of the new political history, which examines voter behavior with a sharp eye to the social bases of politics, Silbey with a high degree of success equalizes the manuscript handicap by skillful use of the tools and insights of political science and of a corpus of voting data. He devotes little attention, however, to ethnocultural issues, which molded party structure in the 1850s, and which the author argues contributed to party stability in the 1860s. And there will be those who will wonder that more space was not given to the factors of slavery and race.

Abandoning conventional labels of War Democrats and Copperheads, Silbey divides the Northern Democracy into Legitimists and Purists. The first sought to establish the idea of legitimate opposition to the administration, but not to the war for the Union, with the hope of attracting conservative voters who were not Democrats. The second stressed the purity of old party principles, sanctioned peace as the means to restore the Union, and feared the Legitimists would degrade the party in the quest for non-Democratic voters. Silbey successfully demonstrates that the Northern Democrats, despite this factionalism, were not fragmented, nor subject to voters' volatility. They drew strength from "a historically determined complex structure of voting behavior and the imperatives of partisanship" (p. 244).

Silbey's extended essay is a scholarly paean to the extraordinary durability of the United States two-party system. Uniquely in the history of the modern world, the United States for nearly a century and a quarter has been administered by the same two parties, albeit with significant realignments, alternating with one another. If "a respectable minority," as a contemporary claimed, and "perhaps, wrongheaded," as Silbey acknowledges, the Northern Democrats during the 1860s contributed to this achievement. His book and its persuasive central argument will be greeted with pleasure by scholars of Civil War politics.

JAMES A. RAWLEY  
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Lincoln*

RAOUL BERGER. *Government by Judiciary: The Transformation of the Fourteenth Amendment*. Cambridge, Mass.: Harvard University Press. 1977. Pp. x, 483. \$15.00.

Raoul Berger joins the list of legal scholars who have considered the intent of the framers of the Fourteenth Amendment in reaction to modern Supreme Court interpretations. According to Berger, the explicit intent of the framers was to merely protect a narrowly defined body of fundamental rights from discriminatory state laws and judicial procedures. The Supreme Court's recent applications of the amendment to segregation, voting rights, and Bill of Rights guarantees are, for Berger, an unconstitutional usurpation of power that he regards as a dangerous threat to democratic government. His purpose is to demonstrate that the court should not make new law, but apply law as its framers intended.

Berger focuses upon the amendment's legislative history. But his analysis fails to consider the original Reconstruction problem of civil rights within the historical context in which the need for protection arose. No explanation is offered as to why civil rights guarantees were adopted. This omission is particularly puzzling since Berger argues that the framers were racists whose overriding concern was to maintain their political hegemony. Yet, he also argues, correctly, that they consciously chose not to provide direct guarantees of the rights that would have contributed most to their political power, namely, the voting rights of Southern freedmen and unionists. The reader is left wondering why the framers protected civil rights when that action contributed so little political advantage and was fraught with potentially devastating political consequences.

Presuming to let the facts speak for themselves, Berger misinterprets and misuses historical evidence. He selectively quotes and misquotes the framers at a number of crucial points. He misquotes Congressman John A. Bingham in arguing that his proposed amendment secured minimal rights when Bingham's comments show that he intended to secure Bill of Rights guarantees. Bingham also stated that the Fourteenth Amendment as adopted reached that end, though Berger ignores this evidence. Berger argues instead that the framers intended to confer upon blacks the natural rights of life, liberty, and property, and their "necessary incidents," which he says the framers understood as minimal rights. They explicitly stated, however, that these natural rights encompassed all of the rights of free citizens that governments were created to secure. Berger further insists that minimal rights were to be protected only from discriminatory state laws and judicial procedures, because the framers never questioned the states' performance in safeguarding freedom. Yet he ignores contradictory congressional statements showing that they were motivated to provide the protection that should have come from the states precisely because

the Southern states refused to do so. He even cites contradictory evidence demonstrating that the framers were also motivated by " 'damnable violence,' 'wrong and outrage,' 'fiendish oppression,' and 'barbarous cruelties . . . ' " committed by Southerners (p. 26). Berger concludes that the negative prohibitions of the Fourteenth Amendment are additional evidence of its restrictions upon the states alone. But he ignores contradictory comments demonstrating the framers' understanding of the negative prohibitions against slavery contained in the Thirteenth Amendment as an open-ended congressional authorization to protect the natural rights of free citizens. Denying that they had such an understanding, Berger inexplicably fails to consider where they found the authority to enact the Civil Rights Act of 1866, which they expressly understood as securing these natural rights. Furthermore, he claims that the act and the amendment were identical when this equation suits his purposes, but that they differed when it does not.

Berger's book is not an objective historical study. It is a lawyer's brief that fails to make its case.

ROBERT J. KACZOROWSKI  
Wagner College

THOMAS HOLT. *Black over White: Negro Political Leadership in South Carolina during Reconstruction*. (Blacks in the New World.) Urbana: University of Illinois Press. 1977. Pp. 269. \$12.50.

During the past decade, a large number of books have appeared, all long overdue, on the role of blacks in Southern Reconstruction. Thomas Holt's *Black over White*, a study of South Carolina's black Republicans, is one of the best of these so far. Well crafted and well written, it not only broadens our knowledge of the period, but also deepens it, something that recent books on Reconstruction have too often failed to do.

Holt is able to accomplish this because he is concerned not merely with who black politicians were and what they did but with how they functioned within the political system. Thus he penetrates beneath the superficial, and frankly pointless, preoccupations over whether black politicians were venal or gullible to see whether they wielded power and exerted influence and, if they did, how and to what ends and against what obstacles.

The investigation of black political leadership in South Carolina focuses on two related facets—its origin and source outside the political arena, that is, its recruitment, and then its composition, mode of operation, and voting behavior in the general assembly. As a result of this analysis, Holt offers

three evaluative observations about the Republican Party in the Palmetto State.

The most convincingly demonstrated of these assertions is that black leaders emerged and then operated quite independently of white supervision and control. They rose to positions of leadership, not through the Missionary Societies or the Freedmen's Bureau, which were both permeated by white personnel and attitudes, but through careers carved out through personal success in the United States Army or the black churches. Then, once elected, black Republicans as a group began, by 1870, to consolidate and, using their numerical majority in the assembly, executed a series of maneuvers whereby they displaced the party's white elite. Employing the influence they possessed on committees and in caucus with as much subtlety and sophistication as Holt applies to analyzing the process in the best chapter in the book, they "discerned the key levers of power in the state and moved to take them over" (p. 121). As a consequence, they gained control over both the legislature and the party's organizational apparatus.

The second of Holt's observations is that the Republican leadership, both black and white, failed to keep the party united and strong, and can thus be blamed for its vulnerability in the mid-1870s. With a huge electoral majority, greater than in any other Southern state, Republicans allowed factions to emerge which biennially on the state level produced bolts, culminating in Governor Daniel Chamberlain's disastrous courting of the moderate Democrats in 1875-76. Even after their electoral defeat in 1876, there was still a good chance for the party to recoup, but instead Republican legislators collapsed under pressure and the party was decimated. Exactly why this was so is not fully explained but, whether enjoying an overwhelming majority or reduced to a besieged minority, South Carolina Republicans were not able to build or consolidate their party.

A suggestion about why this occurred is provided in the third, and main, interpretive theme of *Black over White*. Holt's analysis of black political leadership reveals a split between two groups which he calls "browns" and "blacks." The browns were antebellum free Negroes, often mulatto, who possessed a social and economic status which they valued highly. This, in turn, served as a barrier between them and the majority of Negro legislators and voters who were former slaves with limited education and public experience. Had the browns been more numerous, they would, Holt feels, have probably formed a third social and political force, but, as it was, they were compelled to cast their lot with the blacks. Nevertheless, they operated as a conservative voting bloc among the Republicans in the legislature; invariably they



were opposed to legislation protective of black farm tenants and workers; and finally they provided support for overtures toward the Democrats made in 1874 and again in 1875-76 when Chamberlain led the party to disaster.

In singling out the browns as the Achilles heel in the Republican party, Holt has offered an intriguing hypothesis. But this still would not explain why there was general collapse after the defeat of 1876. Moreover, if a culprit for the calamitous fusion movement has to be found, then surely Governor Chamberlain's employing his vast patronage power and party leadership to effect such an entente would seem to be cause enough. After he took that initiative, there would of necessity be many supporters of the powerful governor in the party, whether black, white or brown; but it was taking this step in the first place that was decisive.

*Black over White* is a fresh analysis and a provocative interpretation of black politics during Reconstruction. Consequently it stimulates comment and criticism and, for that reason, is a very welcome contribution to the literature on Southern Reconstruction.

MICHAEL PERMAN  
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Chicago Circle

ROBERT G. SHERER. *Subordination or Liberation? The Development and Conflicting Theories of Black Education in Nineteenth Century Alabama*. University: University of Alabama Press. 1977. Pp. viii, 214. \$10.00.

The history of black education in the United States has been dealt with only disjointedly. General accounts—such as Carter G. Woodson's and Horace Mann Bond's—are two or more generations old. That by Henry A. Bullock deals with the South and then only in detail with events after 1865. Since the edifice of history grows by bricks or blocks rather than entire structures, occasional specialized works also fill out various aspects of the scene, including a few leading personalities. Major themes of this fitful historiography include striving by selected individuals to gain an education, the immensity of the burden of racism, the relative helplessness of black communities in overcoming this burden, and an imputed civic passivity in the face of governmentally approved deprivation.

Robert Sherer's *Subordination or Liberation* questions the adequacy of some of these themes and views black education from an enlightening perspective. His is the first published work since 1939 to deal with black education in a single state. (The first was Bond's book on the same state.) Since public education is organized around state aid and legislation, this approach enables Sherer to trace the role of state legislation and administrative pol-

icy and analyze efforts of black educators to lobby for various measures.

The author contends that educators of blacks led "through holes in the web of subordination" in a variety of ways (p. 148). First, they refused to adopt industrial education to the exclusion of liberal education. This resistance was practiced in schools organized by black clergymen as well as by the northern-financed American Missionary Association. Booker T. Washington made few converts among his colleagues in the state. Second, blacks were incessant initiators of schools and had constantly to supplement the officially starved public institutions of learning. Third, black educators who conformed to the white ideal of limited education for blacks (such as William Hooper Council) suffered ultimate personal disillusionment or enjoyed prosperity limited to one favored institution (as Washington). Each contention is well documented.

The work is modestly conceived and well executed. Several questions, however, can be raised. The period examined, 1865-1901, seems to be somewhat arbitrary. Sherer's story is so "internal" that the reader gets little sense of larger state-wide political and economic currents. (Bond does this very well indeed.) On balance, Sherer's study is to be welcomed. One hopes he will extend his work into more recent years.

MEYER WEINBERG  
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EDWARD MAGDOL. *A Right to the Land: Essays on the Freedmen's Community*. (Contributions in American History, number 61.) Westport, Conn.: Greenwood Press. 1977. Pp. xiv, 290. \$16.95.

Edward Magdol's thesis is that from 1861 until 1877 Southern freedmen comprised "a landless agrarian laboring class" whose efforts to "gain lands and independence" were shaped by "kinship networks" and an "ethos of mutuality" (p. xii). The first four essays discuss the forms of self-help and mutual endeavor, derived from their African and slave heritage, that enabled freedmen to "take care of themselves." The remaining essays argue that "a working-class leadership" guided "freedmen's communities" and that freedmen's efforts to own land, which began as "agrarian class conflict," (p. 150) generated a "Black Village Movement" that re-emerged in the 1880s as the Exoduster hegira.

Magdol, influenced by Herbert Gutman and critical of Stanley Elkins and Eugene Genovese, is primarily concerned with interpretation. Unhappily, he develops his theory unevenly. At one extreme is a thirty-five-page chapter that surveys



joint black land purchases throughout the South over a seventeen-year period. At the other, is a microstudy of militant local leaders, two-fifths of them artisans, intended to establish black working-class leadership. Even this sample, drawn from a Senate Ku Klux Klan hearing, raises problems, for though it represents five states it denominates only forty-six men as leaders, as defined by participation in one of fourteen different activities (state legislator, militia officer, Union League or Loyal League president, school-teacher, and church class leader among them).

Other imbalances result from Magdol's failure to provide contexts for his findings. If, for instance, an African-derived kinship network distinguished the way in which freedmen bought adjoining farm lots, how did their pattern differ from customs among other preindustrial agrarian American families? Similarly, can the work responses of ex-slaves on the South Carolina Sea Islands be grouped with those prevalent among freedmen elsewhere without noting the former's tradition of task rather than gang labor? Or can Sea Islanders' unusual success in landowning be understood without reference to the white Boston capitalists who bought land on St. Helena's Island, established clear title, and then sold it to freedmen? Indeed, Magdol seems to posit so distinctive and uniform an agrarian lower-class black response that he overlooks its variations and the factors which explain them. Perhaps that is why he also assumes that all prewar free people of color were descendants of white masters and implies that Northern free Negroes made their first efforts for suffrage only during the Civil War.

Historians who bring social science to their work impose on themselves a double burden. They must not only provide the usual historical context of change or persistence through time but also establish the social and economic context of those times. Because Magdol has stinted in both respects, his work disappoints even as it raises questions about the freedmen's experience.

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GERALD H. GAITHER. *Blacks and the Populist Revolt: Ballots and Bigotry in the "New South."* University: University of Alabama Press. 1977. Pp. xviii, 251. \$14.50.

"History is past politics and politics present History." This statement, which graced the wall of the Johns Hopkins seminar room as an inspiration to history students of the 1880s, encapsulates the apparent purpose of Gerald Gaither's study of the political relationship between white Populists and

blacks in the late nineteenth-century South. *Blacks and the Populist Revolt* begins with a brief preface by Gaither that touches upon the use and misuse that present-day politicians make of the Populist heritage and continues with a more extended analysis of the same theme in an introduction by Sheldon Hackney. Gaither concludes his book with a short epilogue that sets up a continuum of Populist ideology from the "conservative" George Wallace to the "liberal" Jimmy Carter. Students of present-day politics should find Gaither's and Hackney's ruminations interesting.

For historians of late nineteenth-century Populism, the main value of *Blacks and the Populist Revolt* is that it offers a concise synthesis of the major themes developed by historians since the 1930s. Gaither's coverage of the Farmers' Alliance, the precursor of the Populist Party, is essentially a condensed version of Theodore Saloutos' *Farmer Movements in the South* (1960). Similarly, the analysis of Populist rhetoric revolves around the questions Richard Hofstadter raised in the 1950s as to whether the Populists were progressive or retrogressive in outlook.

The central focus of the book, however, is a re-examination of C. Vann Woodward's contention that the Populists were uniquely tolerant in their attitudes and actions towards blacks. Although he is reluctant to be too critical of Woodward, whom he calls the "Godfather" of Southern history, Gaither concludes, as other historians did in the 1960s, that the evidence does not substantiate Woodward's thesis of Populist uniqueness in racial practices or views. Yet Gaither contends the Populists were mature, did not "often make blanket indictments of blacks" (p. 77), and were willing to cooperate politically with blacks even though they were against social equality. In short, Gaither attempts to take a mid-position between Woodward and some of his critics.

Perhaps it is due to the inevitable lag between research, writing, and publication, but it must be said that Gaither's study is a product of the 1960s in methodology and conclusions. In addition to his reliance on studies of that decade and earlier Gaither's major primary source is that perennial staple of historians: the newspaper. A quick glance at *Blacks and the Populist Revolt* might lead one to conclude that Gaither is a quantitative historian who has gone beyond the traditional literary evidence. Gaither acknowledges the need to use quantitative evidence, but unfortunately he does not follow his own advice. Despite a fifty-eight-page appendix with twenty-three tables and ten scattergrams donated by Sheldon Hackney, Gaither devotes less than four pages to an analysis of quantitative data. Nor does Gaither show any awareness of J. Morgan Kousser's recent *The Shap-*

ing of Southern Politics (1974) which employs the sophisticated quantitative technique of ecological regression. Gaither should get acquainted with Kousser who most likely will become the new Godfather of Southern history.

It is indeed unfortunate that Gaither, who has the know-how and the available data and technology, unlike historians in the 1960s, did not make a more extensive and complex study of Populist political behavior. *Blacks and the Populist Revolt* serves a useful function as a historiographical overview of Southern Populism, but obviously this should be the beginning and not the end-product of a book of this type.

ROBERT M. SAUNDERS  
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GILES OAKLEY. *The Devil's Music: A History of the Blues*. Reprint. New York: Taplinger. 1977. Pp. 287. \$14.95.

American historians have only recently begun to comprehend the importance of antebellum Afro-American music in the historical reconstruction of slave thought and culture. They have been slower still in using postbellum black music as a source of Afro-American consciousness. As scholarly awareness of the crucial relevance of this body of material grows, books such as Giles Oakley's *The Devil's Music* will prove to be an immense aid.

Oakley is the latest in a series of English authors—the most prolific of whom has been Paul Oliver—who have given us useful studies of black music. *The Devil's Music* is a particularly welcome addition to the literature; it is a handsome book, profusely illustrated with well-chosen and informative photographs. Its text is, for the most part, equally valuable, but Oakley is most erratic in handling historical context. While his sections on black migration and on the Great Depression are excellent, his treatment of slavery is fragmented, occasionally inaccurate, and based upon a bibliography that omits many of the most important historical studies.

His discussions of blues singers and of the development of blues music are another matter. Here Oakley has done his homework in the relevant recorded and published materials and the result is a sensitive and meaningful account. His book is filled with good portraits of such black singers as Charley Patton, Henry Thomas, Leadbelly, Ma Rainey, Bessie Smith, Blind Lemon Jefferson, Lonnie and Mary Johnson, and Muddy Waters. He is perceptive in handling such complex and frequently misunderstood subjects as the development of blues style, the impact of the black urban migration upon Afro-American expressive culture,

the effects of the recording industry on black music, the intricate relationship of secular and religious music, the enduring ties that bound professional and folk music together, and the importance of humor in leavening the message of blues songs.

Although *The Devil's Music* has no explicitly stated theme, one does emerge: the blues, regardless of the numerous changes they underwent, are the record of the culture and consciousness of black Americans and one of the chief vehicles blacks erected for articulating their feelings, recounting their experiences, and providing relief and release for individuals as well as for the group as a whole. Throughout, Oakley comprehends the communal nature of blues music which provided for "the public celebration of shared joys or the ritual expression of inward griefs." Throughout, he stresses the realism embodied in blues expression: "for those who sang it—and still do—it is the music of feeling, of direct observation and statement of what is rather than what might be, unadorned, unelaborated, of no pretension."

While Oakley has written more of a genre study than a fully developed social history, his awareness of the blues as a central medium of group expression and communication makes *The Devil's Music* a fine introduction to a mode of twentieth-century expressive culture which historians can no longer afford to ignore.

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EARL N. HARBERT. *The Force So Much Closer Home: Henry Adams and the Adams Family*. New York: New York University Press. 1977. Pp. xi, 224. Cloth \$15.00, paper \$4.95.

*The Force So Much Closer Home*, its title taken from an 1830 entry in the diary of John Quincy Adams, is an attempt by Earl N. Harbert to assess the influence of the Adams family and its traditions upon the writings of a notable member of the fourth generation, Henry Adams. This is a challenging assignment as both the personality of the subject and the analysis of family import are complicated and require an almost intuitive interpretation of frequently ambiguous or incomplete data. Within the limits set for himself, however, Harbert has succeeded in illuminating at least one facet of the enigmatic Henry Adams.

After identifying what he believes to be the principal intellectual components of the family heritage, the author examines their relationship to four areas of Henry Adams' literary production: 1.) apprenticeship and early writings, including the article on Captain John Smith and the lengthy

review of Lyell's *Principles of Geology*; 2.) the biographies (John Randolph and Albert Gallatin) and the novels (*Democracy* and *Esther*); 3.) the deservedly famous *History*; and 4.) the masterful *Education of Henry Adams*. This format, while well suited to the author's intent of concentrating on literary matters, does omit any extended discussion of other Adams writings, including the incomparable *Mont St. Michel and Chartres* as well as his observations about the need for a scientific history. As a result, Harbert's study has greater appeal for the student of American literature than for the historian. Yet, it is good intellectual history and a pioneering effort in the investigation of family mystique and importance.

Harbert concludes that such Adams family characteristics as the ideal of public service, a belief in progress, and commitments to literature and science were profoundly significant upon the form and substance of Henry's art and philosophy. But, wisely, he also concludes that Henry was forced to go beyond the family inheritance in his search for meaning in the increasing complexity of modern existence. When Adams declared his "education" a failure, he was also saying that the family legacy was not enough. Paradoxically, the *Education*, says Harbert, was "an artistic experiment with words," whose success or failure depended on what the reading audience decided to do with it.

There is much in this book that clarifies the literary craftsmanship of Henry Adams. Particularly useful is Harbert's explanation of how Adams employed personality to exemplify universal and national traits. This insight adds much to an understanding of the *History* as well as the manner in which Adams developed his biographies of Randolph and Gallatin. The book is not a full intellectual biography, but it does sharpen appreciation of a principal American writer and is encouragement for others to pursue the shadowy relationship between artist and family.

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ALFRED D. CHANDLER, JR. *The Visible Hand: The Managerial Revolution in American Business*. Cambridge, Mass.: Harvard University Press. 1977. Pp. xvi, 608. \$18.50.

In this splendid survey of business management within the wider context of business history from about 1840 to World War II, Alfred D. Chandler maintains that the "visible hand" of modern business enterprise rather than the "invisible hand" of market forces came to coordinate the activities of the economy and allocate its resources.

The focus on management distinguishes this volume from other extended treatments of business history, which usually approach the subject from the perspective of individual industries, corporations, business functions, or outstandingly successful businessmen. Although Chandler has collected a considerable amount of his material from government and corporate reports, he relies for the most part on the vast collection of secondary works dealing with the history of business. Since this corpus is of varying quality, the course of his narrative tends to follow the paths already laid out by the scholars in the field who devote attention to management.

Chandler maintains that the techniques of modern management were born with the coming of the railroads, which "caused entrepreneurs to integrate and subdivide their business activities and to hire salaried managers to monitor and coordinate the flow of goods through their enlarged enterprises" (pp. 77-78).

As the railroad network expanded, railroad leaders then embarked on the building of large, inter-regional systems. This process inaugurated a new dimension of management—the organization of multiunit groups as integrated operations. Chandler feels that the most effective example of this development was the divisional line and staff organization largely devised by J. Edgar Thomson for the Pennsylvania Railroad system.

After the turn of the twentieth century, this revolution in management spread swiftly to consumer-oriented businesses such as flour milling, meat packing, and cigarette making. Mass retailing, in the form of department stores and mail-order houses, followed suit. Mass production in heavy industry moved more slowly in this direction because of difficult technological problems. But the solution of these led to modern factory management. Legal restrictions against cartels led to mergers which created the large firms which dominate the business scene today. But to be successful, these had to evolve from horizontal combinations to multiunit integrations moving forward toward marketing and, where possible, backward toward procurement.

The shortcomings of the book are in part caused by omission and in part by a too rigorous insistence upon the thesis. It is doubtful, for example, whether the need for integrated management and pressure from managers themselves played as important a role in producing mergers as the activities of financiers who anticipated large capital gains through stock speculation and control of the market. Chandler also has almost nothing to say about financial institutions, although their growth would in many instances strengthen his argument. Life insurance companies, for example, in ex-

panding their agency and marketing systems, contributed greatly to the development of integrated management.

But perhaps the most notable omission is the absence of any attention to entrepreneurial theory or the personalities and backgrounds of business leaders. Much has been written about the distinction between managers and entrepreneurs which would be helpful to this study, particularly because Chandler devotes such attention to pioneer managers like J. Edgar Thomson.

Yet despite these qualifications, Chandler has written a book which is a model of good organization, clarity of exposition, and comprehensive synthesis. If the primacy of the visible hand remains in doubt, its effectiveness in the moulding of American business is not.

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DAVID F. NOBLE. *America by Design: Science, Technology, and the Rise of Corporate Capitalism*. New York: Alfred A. Knopf. 1977. Pp. xxvi, 384. \$12.95.

*America by Design* is a daring study which well evidences the growing sophistication of radical historiography. Within a relatively brief compass, David F. Noble challenges a number of dicta about the nature of American industrialism between 1880 and 1930 by arguing that the exigencies of corporate capitalism exercised a pervasive influence on the engineering community and that this phenomenon, in turn, manifested itself not only in the creation of increasingly complex technologies but also in subtle attempts to control society. According to Noble, engineers working primarily in new science-based industries played a dual role as agents of American capitalism. Styling themselves as "corporate reformers," they consciously sought to "design" both technology and society according to rational precepts, thereby shaping institutions and values in a manner agreeable to the capitalist ethos of growth, stability, and bureaucratic control.

Noble develops this thesis by showing how late nineteenth-century business mergers resulted in the formation of giant firms like General Electric, DuPont, and AT&T, and how these firms, with their massive technical requirements, soon monopolized the employment of academically trained engineers. Apart from their dependence on big business for jobs, contracts, and the like, engineers also discovered that they shared common ideological bonds with their corporate benefactors. Moved by "a compelling vision of an affluent, humane, tranquil, and powerful America" (p. 64), both groups

held a strong allegiance to ideas of discipline, efficiency, and progress. Thus, once they became enmeshed in the corporate process as managers, educators, and members of professional societies, engineers worked zealously to remove uncertainties not just from the workplace but from everyday life.

"Corporate liberal reform" embraced a broad range of activities, and Noble documents these undertakings in detail. At the technical level engineers doubled their efforts to standardize production methods, control the patent system, and organize industrial and university research. Working through a variety of forums, they also devised strategies that went far beyond streamlining manufacturing operations. The most interesting and original parts of the book address the question of "social production," notably the efforts of engineer-reformers to overcome resistance to their designs. Here Noble emphasizes the role of education, especially the efforts of employers and educators to inculcate "correct" values and attitudes that would fit the demands of the industrial system. This process occurred at two levels, industrial education for the workers and higher education for young engineers. The former sought "to prepare people for a life of labor" while the latter prepared people "for a life of managing labor" (p. 168). Both approaches stressed teamwork, loyalty, obedience, and diligence. At still another level, the introduction of modern management extended and refined these practices by employing the latest methods of psychological testing and manipulation to control human behavior both in and out of work. This "shift of focus . . . from the engineering of things to the engineering of people" (pp. 263-64) exerted a powerful influence on twentieth-century Americans. In a very real sense it defined the features of a new technocratic era.

Although the breadth of Noble's synthesis is impressive, he constructs a model that is a bit too neat and dramatic to account for the inherent complexities of industrial development. One flaw is his misunderstanding of the origins of modern management. As Alfred D. Chandler and others have pointed out, multi-unit enterprise with its bureaucratic hierarchies, salaried managers, accounting techniques, statistical controls, and forecasting methods first appeared in the trunk-line railroads of the 1850s, not in the electrochemical industries of the 1890s. Modern factory management with its emphasis on time-accounting, work specialization, and standardization dates back even further to the firearms and textile industries of the post-War of 1812 period. Indeed, in its American context, the ideology of industrialism had a striking affinity with "republican" ideology of the early national period.



What was different about corporate capitalism after 1880 was the growing speed, scale, and complexity of industrial production and the obvious need for coordination and control. These developments called forth more sophisticated methods of management which, as Noble indicates, began to pervade a much broader spectrum of society. The novelty of corporate reform thus rested with the fact that academically-trained engineers began to address problems of a different magnitude.

Similar criticisms can be made of Noble's emphasis on the role of science in industry. Contrary to his view, "scientific ideas" did not "give rise" to industrial manufacturing nor did they underlie precision workmanship and standardization (pp. 71, 82, 118). Indeed, the very industries that pioneered these advances were not science-based but managed and controlled by people who came from "shop culture" backgrounds in the textile, firearms, and machine-tool industries. Only gradually did shop methods give way to science-based engineering in metalworking and other manufactures. Although Noble discusses this problem, he seems to underestimate its significance as a defining feature of the new corporate era.

Other flaws in the argument betray a casual acquaintance with the recent literature on early nineteenth-century technology. Eli Whitney, for instance, did not produce muskets with interchangeable parts (p. 76), Alexander D. Bache did not found the Franklin Institute (p. 22), and William Sellers was only one of many who "laid the basis for the modern machine-tool industry" in America (pp. 71, 76). These and other inaccuracies and omissions are lamentable because a more exacting assessment of antebellum technology would have clarified and strengthened the book's main thesis.

Noble has produced a thought-provoking and instructive study that sheds new light on technology as a social process. Not everyone will agree with his conclusion that modern technology has become "merely a means to corporate ends, a vehicle of capitalist domination" (p. 321). But everyone will benefit from the probing questions he asks and the engaging manner in which he interprets a key period of American history.

MERRITT ROE SMITH  
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LEONARD DINNERSTEIN and DAVID M. REIMERS. *Ethnic Americans: A History of Immigration and Assimilation*. New York: New York Univrsity Press. 1977. Pp. xv, 184. \$12.00.

MAXINE SELLER. *To Seek America: A History of Ethnic Life in the United States*. Englewood, N.J.: Jerome S. Ozer. 1977. Pp. x, 328. Cloth \$13.95, paper \$6.95.

Despite Will Herberg's assertion in the 1950s that religion had replaced ethnicity as the basis of personal identity in America, it has become apparent in the 1970s that Americans more than ever think of themselves in ethnic terms. The two surveys under consideration reflect not only this renewed interest in immigrant studies but also the new directions that such studies have taken. Four decades ago, Oscar Handlin shifted the question historians were asking from "What have immigrants done to America?" to "What has America done to the immigrants?" Today historians and sociologists have combined their skills to explore the ways in which ethnic groups have adjusted to, and in the process shaped, American society.

With their emphasis on the twentieth century and their convenient tables, which carry statistics of immigration down to 1973, Leonard Dinnerstein and David Reimers have provided a useful addition to earlier surveys, which of necessity concentrated on the nineteenth century. Their chapter on Spanish-speaking minorities, for example, presents valuable material on a subject not often covered in other texts. The authors have, moreover, asked the right questions, stuck to central themes, and livened their text with dramatic incidents and interesting data. The reader is not likely to forget the story of the horrors that awaited many of the newcomers on reaching America. With all its strong points the book has liabilities as well. It is too brief, too superficial, and too derivative to make any scholarly contribution to the study of immigration. The story of the "Old Immigrants," told in only twenty-five pages, no more than summarizes earlier accounts of nineteenth-century immigration. Though lively, the style sometimes detracts from the message, as, for example, in the first chapter where the phrase "dominant culture" or "dominant group" is used four times on one page. But even with these limitations, *Ethnic Americans* is well-worth considering as supplementary reading in introductory survey courses.

Maxine Sellers' *To Seek America* offers similar possibilities as a basic text for courses in American social history. For the first time teachers have available a text of some three hundred pages that successfully relates American history in terms of ethnicity. Trained as an historian, Sellers never allows her readers to lose touch with political historical developments while she is exploring the immigrant experience. Themes such as Jacksonian Democracy, Manifest Destiny, and Progressivism all receive their due and are nicely woven into the overall themes of ethnicity. Sellers also deals with basic sociology, as, for example, in her opening chapter entitled "Ethnicity: What is it? Why is it important?" Historians and sociologists will no doubt find the treatment of the historical move-



ments and the sociological concepts elementary at times, but the combination results in a successful textbook.

A sample of Sellers at her best appears in her chapter on "New Frontiers." She weaves together the story of three nineteenth-century movements—the trans-Atlantic migration, the westward movement, and the rise of an urban frontier—never once losing touch with her central theme, the immigrant experience. In the following chapter Sellers explains how these three interrelated movements led to the development of an American style based on ethnicity. Later in the book she uses the same technique to describe the ethnic history of America between 1880 and 1924, first describing the migrations in one chapter, and then analyzing the institutions that followed in several others. After two chapters on new generations and new immigrants in the last half-century, the author concludes with another sociological chapter dealing with "The New Ethnicity."

Sellers' study has its flaws. Like any survey, the text is often superficial—as in this overstatement: "When Jacksonian democracy opened the political door to the common man, the urban Irish rushed in and took over." The political door was opened in many communities well before the days of Jackson, and in many others the Irish did not take over until long after Jackson was gone. But Sellers can also turn a neat phrase; she defines "the two distinguishing principles of national identity" in the early nineteenth century as "republican democracy and militant Protestantism." She also notes correctly that George Bancroft considered the American Revolution "the most momentous event since the birth of Jesus."

College or secondary school teachers looking for new texts for their courses on American history should look carefully at *Ethnic Americans: A History of Immigration and Assimilation* and *To Seek America: A History of Ethnic Life in the United States*.

DONALD B. COLE

*Phillips Exeter Academy*

RANDALL M. MILLER and THOMAS D. MARZIK, editors. *Immigrants and Religion in Urban America*. Philadelphia: Temple University Press. 1977. Pp. xxii, 170. \$10.00.

Growing out of a series of symposia held at St. Joseph's College in Philadelphia to commemorate the Bicentennial, this stimulating collection of exploratory essays on eight ethnic groups focuses on "the relationship between religion or religious beliefs and the ethnic experience in post-Civil War urban America" (p. vii).

The first six essays, on Catholic ethnic patterns, are nicely suited to the spirit of broad inquiry that evidently informed the editors' conception of the book. Open-ended, speculative, and on occasion spiritedly argumentative, these schematic case histories extend virtually from the beginnings of immigration for each group to the present. In the opening chapter, Josef J. Barton imaginatively ponders the vicissitudes of core Czech Catholic continuity from homeland peasant village to inner city to suburb as expressed in family, kinship, neighborhood, and small associational styles. Based on the application of ingenious and highly selective sample interviewing and other techniques for Chicago, Barton's essay provides the reader with a tantalizing glimpse into the identity of a virtually unknown American ethnic group, portrayed conventionally as "church Czechs" or "club Czechs." In a complementary study, Rudolph J. Vecoli feelingly depicts the impact of church personnel management practices on the ancient folk religious beliefs and devotional patterns of Italian Catholics of the *Mezzogiorno*.

Unlike Barton and Vecoli, who are concerned with the high tensile gossamer threads and vintage flavors of local and family ethnicity which do not lend themselves to conventional historical documentation, the authors of the other essays concentrate on depicting the formal ethnic institutions reflecting the struggle for ethnic autonomy that came to frustrate the Catholic impulse to unity and uniformity. In a wide-ranging essay, Dennis Clark points to Victorianization, Romanization, and bureaucratization as the processes that turned Irish Catholics into Irish-American Catholics, whether as builders and managers of an American Catholic Church empire or as functionaries of the twentieth-century urban corporate infrastructure. Drawing on the rich and varied recent literature of Anglo-Irish-American social history, Clark makes increasingly intelligible the transformation of the nation's classic ethnics into the most Anglo-Protestant, ironically, of America's Catholics. In a sweeping survey, Jay P. Dolan deftly portrays first, the rise of the German national parish, beginning in Philadelphia in 1789 with what was the first example—if not quite a prototype for the ethnic parishes to follow; next, the long-term German rivalry with the numerically and otherwise more favored Anglo-American Irish; and finally the dissolution of the first of the nation's continental Catholic subcultures. In a review of the emergence of the Polish national parish, William J. Gallush accords special attention to "the transmitter of Polishness" (p. 93), the parish school, in "the golden era of Polonia" (p. 93) of the interwar years. Gallush concludes that Catholics of Polish descent, the second stage in Will Herberg's tri-

stage typology of ethnoreligion, rather than the first-stage Polish Catholic, has increasingly become the prevailing pattern for maintaining loyalty to the Polish heritage. In the book's most tendentious but illuminating essay, on the Slovaks, M. Mark Stolarik proposes the legitimization of a Catholic alternative to the prevailing individual success/social-mobility/"Protestant ethic" American categorical imperative. Instead of the rootlessness, family dissolution, and community irresponsibility syndrome mandated by current social policy, this collective ethic, he argues, would sanction the values of group loyalty, of the extended and stable family, and of the ethnic neighborhood.

The last two essays, excellent in their own terms, do not quite fit the book's structure and seem to be included so as not to appear to exclude non-Catholic ethnics. Robert Mirak's fascinating analysis of the adaptation to the American scene by the most venerable continuous independent Christian ethnic church in the world through the offices of the rival Armenian Orthodox and Armenian Protestant churches is tightly delimited to the quarter-century before 1915, with only a passing allusion to later developments. Maxwell Whiteman's circumscribed study, subtitled "a Philadelphia fragment," effectively highlights the pioneer role of Rabbi Sabato Morasis as an arbitrator in 1890 in a strike of East European Jewish garment workers against their German-Jewish employers, but barely suggests the distinctively religious dimension of Jewish ethnicity.

In this genuinely provocative "reconnaissance" into the historic experiences of primarily ethnic Catholics, the temptation to overcompensate for the real neglect of these groups has inevitably led to a certain loss in perspective and some glaring blindspots. In the introduction the editors might have taken the opportunity to sketch out the contours of the total ethnic and religious urban scene rather than polemicizing for pluralism and against assimilation in what has become a stale exercise. Surely German and Scandinavian Lutherans and Baptists, Orthodox Greeks, Africans, Chinese, Mexicans, and many others for whom religion has been a central experience also converged upon the cities and shared in a common if not identical adventure that challenged their capacities to endure. Despite a justifiable emphasis on continuities, the editors ought also to have taken greater note of the powerful forces making for discontinuity, such as wars, depressions, labor, class, and generational conflicts, and most recently, the "revolutionary transformation," as John Tracy Ellis has called it, unparalleled since the Reformation, in the Catholic Church and in the attitudes and life-styles of its clergy and communicants at least since the Second Vatican Council. Is it not signifi-

cant that not a single contributor to this book is a clergyman? But these caveats are by no means intended to detract from the importance of a most welcome and thoughtful book that makes a genuine contribution to the complex unravelling of what it can mean to be an American.

MOSES RISCHIN

*San Francisco State University*

DANIEL MERLE PEARSON. *The Americanization of Carl Aaron Swensson*. Rock Island, Ill.: Augustana Historical Society. 1977. Pp. xviii, 169. \$5.95.

Carl Aaron Swensson, the son of Swedish immigrants, became the pastor of the Bethany Lutheran congregation, Lindsborg, Kansas in 1879. He founded Bethany College on October 15, 1881 and spent the rest of his life in that work. He died on February 16, 1904.

Daniel Pearson's book deals with the acculturation of Swensson. He uses Milton Gordon's model of an ethnic group, which, Pearson says, "usually establishes its identity by race, religion or national origin." Complete assimilation is impossible because of "the existence of social and religious discrimination and prejudice in American society" (p. xii). Pearson feels that this model explains why "Swensson could thus give up, or rather amend, his adherence to pietistic religious values and accept the Republican party, middle-class emphasis on order and prosperity" (p. xiv).

The study indicates that Swensson struggled with the tensions between his love for Sweden and for America and between pietism and power. He could appeal to his fellow Swedish-Americans that it was "the special duty of our Swedish-American schools . . . to pay all possible attention to the Swedish language, Swedish literature and Swedish history." Yet, when he spoke to Americans, often in the quest for sorely needed financial support for the college, he could say that "all Swedes were born Republicans" and that "the second crop of Swedes is always thoroughly American."

It is with the second tension between pietism and power that Pearson's study especially deals. Pearson believes that Swensson's ideals of pietism were in large measure sacrificed for power; he "based his political affiliation less on moral principles and much more on the basic question of power" (p. 85).

It would have been helpful if Pearson had defined pietism more precisely. He seems to regard pietism in terms of individualism, whereas, in fact, it is much more: spiritual regeneration, simple and disciplined living, a desire for pure church communities, and emphasis upon biblical authority. Also, Swensson may not be as guilty of moral lassitude

as Pearson suggests. Swensson rather reflects the tendency of pietism to sanctify the status quo as established by dominant power structures. Historians such as Sidney E. Mead have persuasively shown this to be the case. If this is true—and I believe it is—Swensson's behavior is a reflection of a deficiency in the world-view of pietism, not of a lack of personal integrity.

Pearson's book is a valuable contribution to the study of acculturation and to Swedish-American history. His diligent research is to be commended.

EMMET E. EKLUND  
*Pacific Lutheran University*

H. BRETT MELENDY. *Asians in America: Filipinos, Koreans, and East Indians*. (Immigrant Heritage of America Series.) Boston: G. K. Hall. 1977. Pp. 340. \$9.95.

*Asians in America: Filipinos, Koreans, and East Indians* is an important addition to the literature on Asian-Americans. Focusing on the experiences of immigrants from the Philippines, Korea, and the Indian subcontinent, H. Brett Melendy's study provides a survey of three groups which, until recently, have received little scholarly attention. With the revived interest in ethnicity and the marked increase in Asian immigration since the 1965 immigration law, historians have endeavored to trace the political, social, and economic development of the older Filipino, Korean, and East Indian groups which evolved from a relatively small number of immigrants, most of whom arrived in the early years of the twentieth century. Melendy, whose research on Filipinos in Hawaii and the mainland United States represented a part of this scholarly interest in the less numerous Asian-American communities, has reviewed thoroughly the primary and secondary sources and presents his findings in a clear, straightforward style. The result is a book that will not only benefit scholars, but should also be useful in courses on the history of Asian-Americans.

Each of these immigrant groups consisted principally of agricultural laborers. Filipinos and Koreans migrated mostly to Hawaii before many left there to seek better economic opportunities on the mainland; East Indians frequently entered the United States via Canada. They all experienced discrimination, were denied United States citizenship, and were subjected to stereotyped characterizations by exclusionist groups. By the 1920s, legislation had virtually ended immigration from India and Korea, while the Tydings-McDuffie Act of 1934 limited immigration from the Philippines.

Melendy, however, provides more than a chronicle of white racism. He has fully and judiciously

used the limited number of sources that provide insights into life in the small Filipino, Korean, and East Indian communities. In addition, he discusses their political activities, especially the Korean independence movement and the Indian nationalist agitation during World War I; both immigrant groups, while united on the objective of independence for their homelands, were divided over programs and strategy.

World War II forced reconsideration of America's treatment of Asian groups with the result that in 1946 Filipinos and East Indians were at last granted naturalization privileges, but it was not until 1952 that Koreans became entitled to citizenship. Yet immigration from the Philippines, Korea, and India remained limited; as a modest concession to Asian nationalism, each was granted a small annual quota of immigrants. With the passage of the 1965 immigration law, immigration from these areas has become significant. Whereas in 1960 less than five thousand immigrants arrived from these countries, by 1974 the total was over seventy-five thousand.

While providing a summary of three groups of Asian-Americans, Melendy's book treats each separately and one wishes that he might have offered an integrated comparative analysis of their experiences. Also, the political and racial atmosphere in early twentieth-century America, especially in California, and the influence of the well-established anti-Chinese and anti-Japanese sentiment on these smaller immigrant groups seem to warrant attention. Overall, *Asians in America*, as a carefully researched comprehensive treatment of an important topic, contributes to an understanding of Asian-Americans.

GARY R. HESS  
*Bowling Green State University*

ROBERT M. IRELAND. *Little Kingdoms: The Counties of Kentucky, 1850-1891*. Lexington: University Press of Kentucky. 1977. Pp. ix, 184. \$11.50.

In this slender book Robert M. Ireland extends his study of the county in Kentucky down to the drafting of the state's fourth constitution in 1891. In neither this nor his earlier book on the subject has he concerned himself with the kinds of materials which have cluttered the usual county histories. He has considered the county as an organic socioeconomic-political branch of Kentucky government. It is somewhat remarkable that as an institutional organism the county in Kentucky has maintained many of the archaic features long ago abandoned in earlier British and Virginia forms.

This grass-roots sector of applied government has historically been a central fact in the social and

political life of Kentucky. County government with the centralizing forces of the courthouse, the trading town, and the first line of government contact for the people has collectively exerted a greater direct influence than the state government itself. Ireland refers to the presiding county judges as "little kings," and so they are. They have historically gathered around them powerful clusters of supporters who have formed the power eye of the courthouse rings. These local rings have reached downward to the people as almost unassailable rural bosses, and upward as decision-makers to the statehouse in Frankfort.

By 1850 the General Assembly had subdivided the state's somewhat limited geographical area into a hundred counties. At least sixty of these were unable from the outset to sustain themselves fiscally, but this in no way lessened the potential influence of their courthouse officialdom or kept them from voicing strong political support or disapproval of state officials. Lack of effective and responsible fiscal management is a subject which almost defies analysis. State finance was fraught with ignorance, flagrant rascality, state government irresponsibility, and perpetual evasion of duties. The subject of assessment of property taxes alone was as confused and debauched as any governmental act could possibly be. Almost as significant and unbelievable as the careless and dishonest management of fiscal affairs was the inability of most Kentucky counties to build and maintain roads, to subsidize bridges and ferries, and to give aid to the building of railroads. Finally it has to be said that the failure to support adequate schools was the capital crime of the counties.

Ireland gives attention to the county seat and its monthly court day as a trading day in which the old English market day was perpetuated. He has examined a massive body of virginal records, and has been able to view the county as a practical but almost always imperfect link in the democratic system. There are none of the maudlin sentimentalities which have marred county history—no ancestors, no genealogy, no "cotehouses."

This book is local history of small bore, but its implications are broad. It reflects objectively the spread and application of what Thomas Jefferson proclaimed as the basic foundation of applied American democracy. Too long the county as a key institutional fact in American rural history has been neglected. This book is a competent seeding of this important field for scholarly consideration.

THOMAS D. CLARK  
*Lexington, Kentucky*

MARTIN J. SCHIESL. *The Politics of Efficiency: Municipal Administration and Reform in America, 1800-1920.*

Berkeley: University of California Press. 1977. Pp. ix, 259. \$11.75.

This book explores the campaigns by reformers and municipal administrators to supplant the inefficiency, patronage, and bossism of nineteenth-century city governments with bureaucratic methods presided over by strong administrators. The best feature of the book is that it painstakingly fills in a convincing line of evolution from nineteenth-century Mugwump campaigns for good government and civil service reform to twentieth-century movements for commission and manager charters. Martin Schiesl argues that strong mayors like Chicago's Carter Harrison and New Orleans' Joseph Shakspeare persuaded reformers that they could accomplish their original goals of strong administration and the removal of partisan influence over local policy without their original methods of pristine nonpartisanship. He maintains that the adoption of new fiscal and research methods allowed local administrators simultaneously to deliver efficiency to middle-class reformers and social services to the majority. Political bosses did not retard this process of bureaucratization; they simply adapted to it. Reformers were also adaptable. When reformers' inventions, like commission government, failed to achieve their goal of unified administration by injecting competition between departments, reformers quietly abandoned their experiment in favor of the manager form of government.

Schiesl advances his theme with the sheer weight of his excellent, brief descriptions of administrative innovations like New York's Board of Estimates and Bureau of Municipal Research, of groups like the National Municipal League and the Census Bureau, of pivotal administrations like Boston's Quincy, Cleveland's Johnson, Detroit's Pingree, Dayton's Waite. Throughout he is remarkably judicious.

He does a much better job of describing reformers' ideologies and programs than he does of analyzing the social and political processes that produced them. The book's greatest strength is also its greatest weakness. It is a book about how leaders—bosses, reformers, administrators—implemented their visions for controlling majorities. The majorities feel, say, and do nothing. Although Schiesl is ostensibly concerned with the interaction of democracy with efficiency at the beginning and end of the book, democracy appears only in the conceptual shadows. We do not learn what the bulk of urban residents wanted from their governments, and hence we have no criteria to determine whether programs or administrators were democratic. Schiesl repeatedly claims that this administrator or that program alleviated the "social" or



"humanitarian" needs of the "masses"—needs that are variously identified as paving streets, building schools, expanding parks, regulating utilities, redistributing taxes, inspecting homes and factories, improving sanitation, providing recreation, but he never tells us whether any of these needs were more popular with particular groups at particular times and places or whether preference for one or a combination of needs led groups to favor one or another kind of governmental structure.

Schiesl similarly underestimates the voters' contribution to the context of these campaigns. He notes that the commission form of government became a fad after Des Moines (1907), and not Galveston (1901), and that a novel Des Moines feature was the inclusion of initiative, referendum, and recall, but he does not recognize that a great many urban residents, particularly labor representatives on character-drafting committees, embraced the Des Moines Plan because they wanted the unprecedented direct democracy and did not care about the unprecedented efficiency. Administrators who lost their jobs through the resulting recall elections, including some in Des Moines itself, or who saw their plans stymied by initiative and referendum elections, would have understood that voters played major parts in shaping the contexts for these issues. Only recently, for example, the city manager of Columbia, Missouri, protested against "the misplaced faith in pure democracy" when public opinion forced him to scuttle his major traffic program. This omission is strange, since Schiesl himself seems to identify at the end of the book with those "who favor 'participatory democracy' over efficiency experts" (p. 197).

DAVID P. THELEN  
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Columbia

MICHAEL P. WEBER. *Social Change in an Industrial Town: Patterns of Progress in Warren, Pennsylvania, from Civil War to World War I*. University Park: Pennsylvania State University Press. 1976. Pp. xiv, 185. \$12.50.

Michael Weber uses the quantitative techniques of the "new social history" to examine the impact of industrialization and urban growth upon a small northwestern Pennsylvania town. He examines the changing social position of manual workers between 1870 and 1910 in Warren, a community that was transformed from a placid, rural market village of 2,000 inhabitants into a "dynamic progressive industrial community" (p. 27) of 11,000 when oil was discovered nearby in 1875 and industry sprang up. After calculating rates of persistence,

occupational mobility, property acquisition, and wealth accumulation for unskilled, semiskilled, and skilled workers from each decennial year from 1870 to 1900, Weber concludes that opportunities for occupational and economic mobility were "significant" and "impressive" for the minority who remained in the city for any length of time. Types of available blue-collar employment, however, changed from small shops to large, specialized firms. This trend toward specialization and large size was accompanied by similar rearrangements in government and in the spatial ordering of the city. Municipal functions expanded and became professionalized, and the preindustrial spatial heterogeneity developed into specialized commercial and industrial sectors and occupationally and ethnically identifiable residential areas. The altered pattern of municipal administration decreased the access of manual workers to the city council, but blue collar employees continued to win election to the Board of Education. In short, Weber concludes, the Warren experience indicates that "Warren's manual laborers could justifiably view America as a land of opportunity" (p. 137).

Weber wisely has opted for the comparative approach, modeling his methodology particularly upon Stephan Thernstrom's Newburyport study. He measures his findings against those of other published studies of social and geographical mobility, and he makes judicious use of the insights of others (notably Herbert Gutman, Paul Kleppner, and Robert Wiebe) to help explain the Warren experience.

Despite these sensible strategies, Weber's optimistic conclusions may not stand up under close scrutiny. For example, two of the three "specific but separate" variables (property and wealth) are so seriously confounded ("most property . . . was in real estate" [p. 169, n. 3]) that they do not deserve separate chapters. Weber uses property assessors' books, moreover, to determine the value of real and personal property after 1870 (but not in 1870), but he neither separates personal from real property in his analysis of wealth accumulation nor wonders what effect mortgages might have had upon a property owner's net worth (deed books would have provided more reliable data on both actual value of real estate and extent of mortgaging). Weber also comments repeatedly upon the positive relationship between long-term persistence and improvement in social status, and he concludes that "selective outward migration operated to improve the rates of mobility by removing a disproportionately large number of the least successful workers" (p. 139). Unfortunately, he does not exploit his data to demonstrate the relationship. Numbers used to calculate "social mobility" in Table 7-6 differ from the numbers used in ear-



lier calculations (e.g., Table 7-6 shows 297 skilled workers remaining ten years, whereas Table 7-4 shows 422). Finally, Weber's methods of calculating mobility largely predetermine his optimistic conclusions. Computing occupational mobility from the workers' initial occupations in the city, for example, supports the optimistic view; if Table 4-6 is accurate, however—and it seems to be suspect—a different method actually shows that in only one decade did more skilled workers rise than fall in job rank. Similarly, the categories for gauging social mobility permit only "No Mobility" or gradients of upward movement. Any downward mobility is submerged in the No Mobility category. These and other flaws will leave readers ambivalent.

RICHARD J. HOPKINS  
Ohio State University

ODIE B. FAULK. *Dodge City: The Most Western Town of All*. New York: Oxford University Press. 1977. Pp. xi, 227. \$9.95.

Some who know my book, *The Cattle Towns*, might wonder that it merits only three citations in the present volume, inasmuch as some of Odie B. Faulk's passages (e.g., pp. 180-87) will seem familiar, although keyed to primary sources alone. Have we here a case of that undergraduate usage, the unacknowledged crib? The trick, one will recall, is to leapfrog a good secondary account and gain a spurious credibility by citing the original sources, thus making the earlier author's reading of the originals—and his footnotes—one's own.

Let the reader compare, as just one example, Dykstra (pp. 106, 107n) with Faulk (pp. 97, 205n) on the existence of racially segregated dance-halls in Dodge City. He will discover not only that the brief passages are remarkably similar in wording, but that footnote citations are to exactly the same eight issues of exactly the same three Dodge City weekly newspapers, listed in exactly the same order. Coincidence? I would like to see the odds.

The cribber sometimes trips himself up, however, as we all know. Faulk's page references to Joseph McCoy's *Historic Sketches* (pp. 55-56, 204n) are in error; but then I warned earlier that I was employing the 1940 Bieber reprint rather than McCoy's 1874 original. Perhaps the most interesting instance of this sort occurs where Faulk (p. 83) purports to quote directly from an 1880 circular to Texas cattlemen, as I had done. Here he committed an amusing mistake, copying from my book (p. 156) not the correct footnote (number 9) but the next one down, the wholly irrelevant contents of which became Faulk's note 3, p. 205.

Not being as cozy with the original sources as he

implies, Faulk makes factual errors, most of them small, since this book is largely anecdotal. To be sure, his chapter six trivializes my insights into the complex political conflict of 1877-80 by prematurely calling the issue moral reform, but it hardly seems worth disputing. What ought to interest the professional historian, I think, is not the bland substance of this little book but its odious methodology.

ROBERT R. DYKSTRA  
University of Iowa

PARK DIXON GOIST. *From Main Street to State Street: Town, City, and Community in America*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. 180. \$12.50.

This important study of community in the years after 1890 is cast in terms of a cultural "dialogue" or "debate" about the meaning of community in an urbanizing society. A number of voices contribute to the debate, including individuals from the fields of literature, journalism, city planning, sociology, and social work. Part one, which deals with the image of the town, argues that the writers who idealized the town maintained the myth that has been dominant to the present—that the small town is the ideal community. Even critics of Main Street, like Sherwood Anderson, retained an image of the town-as-community, albeit a flawed one. In part two, the longer and more intricate section, the focus shifts to those who tried to cope with the transition to the city. Included here are novelists who saw in the city the antithesis of community and, more important, writers and activists who sought forms of community appropriate to urban life. The book gains momentum as it focuses on those who freed themselves from the small-town model of community and formulated an urban-oriented concept to replace it. Released from small-town nostalgia and the need to create older forms of solidarity in the city, these figures looked to new forms of organization to attain community—the urban press, the radical labor movement, and urban planning, to name a few. Here Park Goist takes issue with a number of historians who have stressed the anti-urban image of community in American culture. Except in the section on Jane Addams, he makes a strong case for the emergence of a more complex vision of community than the small-town model allowed for. This line of reasoning is persuasive; it also corrects the present tendency to underline the regressive features of post-Civil War social thought. Instead of writing off earlier thinkers as hopelessly dated, he regards them as important contributors to the ongoing debate about community in America.

The approach to intellectual history taken here has the advantage of anchoring themes in a social context and, in part, overcoming the false dichotomy of "ideas" and "reality." This is done by creating an interplay between the social or cultural situation and the efforts of thinkers to cope with that situation. Ideas are not free-floating abstractions but specific ways of coming to terms with the change from a nonurban to a predominantly urban society. The meanings given to the term community reflect the personal experiences of a select but representative group of thinkers and activists who coped imaginatively with that change. Further, it is suggested that these experiences and the devices for dealing with them were shared by many people (pp. 6, 9). Whether large numbers of Americans used the imagery provided them to make sense of their own situation is a moot point. An inquiry into popular culture would be needed to substantiate this argument. But the use of a variety of sources and the discussion of advertising as an appeal to popular values indicate the pervasiveness of the concern with community and the persistent nature of the search for it in modern America.

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JAMES G. BURROW. *Organized Medicine in the Progressive Era: The Move Toward Monopoly*. Baltimore: Johns Hopkins University Press. 1977. Pp. ix, 218. \$12.95.

In his *AMA: Voice of American Medicine* (1963), James Burrow gave a third of his attention to the pivotal progressive years, 1900 to 1917, to which his new work is exclusively devoted. The two books differ most on the geography of action—and hence the sources relied upon, somewhat on the problems given key emphasis, and least on interpretive point of view. Both books reveal extensive, imaginative research and sensitive, judicious analysis.

In the revolution that converted the American Medical Association into the powerful structure it became, events on the state level, Burrow argues, equaled if they did not exceed in importance those occurring at headquarters in Chicago. Using as sources the printed records of some twenty regionally dispersed state medical associations, Burrow reveals how physicians organized themselves efficiently and lobbied successfully for laws to forward their goals. The "public relations record" set by chief AMA organizer Joseph N. McCormack, Burrow asserts, outdid that of Ivy Lee, and was not "even remotely matched" by any "contemporary spokesman for . . . business enterprise or . . . other professional organization" (p. 23).

Burrow presents a balanced view of physician motivation, finding both sincere altruism promoting public health causes and concern for economic self-interest. Inflation cut real income in a crowded profession, so restricting entrance by enforcing higher educational standards did limit competition. Yet many physician graduates of proprietary schools, practitioners of both old and new medical sects, as well as outright quacks, fell abysmally short of meeting the standards of the new scientific medicine. Hence curtailing their activities advanced the public health. Burrow presents fresh light on the famous Flexner report. State societies took the lead in supporting food and drug legislation, improving milk purity, achieving improved vital statistics, and in many other useful campaigns.

Even such issues as contract practice and social insurance, matters offering a threat to the private practice of medicine, found champions in state medical societies at the height of the Progressive period. In 1917, however, conservative forces became ascendant. The danger of the fraternal order as middleman between doctor and patient seemed more ominous. Government, just recently the chief agency in securing reform, now looked less benevolent. Health insurance proposals portended the "Germanizing" of medical care.

The AMA, securing great power at state and national levels during the first years of the century, had wielded it in many ways so as to mature the profession toward the competence required by the new levels of science. This had been done within a view of the outer world dictated by the perspective of private practice. Complex social burdens cast up by advanced technology, Burrow suggests, could not adequately be assayed from this perspective. Considering these growing problems and the AMA's continuing power, grave tensions became inevitable in the post-Progressive nation.

JAMES HARVEY YOUNG  
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ERNEST C. BOLT, JR. *Ballots Before Bullets: The War Referendum Approach to Peace in America, 1914-1941*. Charlottesville: University Press of Virginia. 1977. Pp. xvii, 207. \$15.00.

Historians of American foreign relations are familiar with the Ludlow amendment, an abortive attempt in the late 1930s to require a popular referendum before the United States could go to war against foreign nations. Less well known is the relationship between grass-roots support for the war referendum idea among peace and isolationist groups and its promoters in Congress, and the lobbying efforts of the Roosevelt administration to

defeat the measure. Ernest C. Bolt, Jr.'s monograph skillfully details the public debate and legislative maneuvers on Representative Ludlow's proposal, but his account is made more persuasive because it is placed within the context of the war referendum idea back to 1914. Indeed, about two-thirds of the book deals with the pre-1930 decades, and the author's coverage of the war referendum approach to peace during the period of American neutrality in the First World War is particularly thorough and stimulating.

Bolt also provides fresh information on the failure of Secretary of State Hughes' proposal for a security pact based on the war referendum idea as a solution to Germany's financial crisis of the early 1920s. The unstated implication of Hughes' efforts is that American involvement without firm commitments in foreign relations in that decade resulted in muddled meddling, and more satisfactory results might have ensued if the United States had intervened more actively or returned to stricter isolationism forcing European nations to rely on their own talents to resolve their financial and political difficulties.

The author asserts that he "made a conscious effort to avoid magnifying the war referendum story out of proportion" (p. xvii). A major theme in fact is that the idea of a direct vote of the people on war declarations never stood alone but was usually espoused along with antipreparedness, civil liberties, liberal pacifism, internationalism, opposition to war profits, or neutrality legislation. This affinity with (and frequent subordination to) other national movements and interests may account for historians' neglect of the idea. Bolt resists the temptation of chastising historians for their unconcern with this issue in the text of his book, but many of his footnotes clearly document the failure of historians to mention the war referendum in their books on pacifistic individuals, peace movements, or related developments in the interwar period.

The author indicates the analogous relationship between the war referendum and the initiative and referendum in the Progressive reform movement but does not offer any critical perspective on the merits or limitations of these forms of direct democracy. Indeed, the book is devoid of any internal analysis of socialist or democratic ideas. Nor does it define "pacifism," "radical," or "progressive reform." Finally, the author may concede too much to the opponents of the war referendum when he suggests that "America could not be both a partner to international peace and domestic progress without assuming its share of the responsibility for maintaining peace" (p. 188). As his own account shows, the war referendum proponents suggested plausible alternatives to full-scale Amer-

ican involvement in the First World War, and American responsibility was not obviously tangible but defined and promoted in broad, ideological terms only by President Wilson and his followers.

In sum, though *Ballots before Bullets* is well researched and written in a clear, straightforward style, it would have been a more provocative book if the author had been more consistently analytical and had attempted to sustain a critical argument evaluating the war referendum idea in the interwar years and its possible relevance for our own day.

DAVID S. PATTERSON  
Colgate University

JAMES STUART OLSON. *Herbert Hoover and the Reconstruction Finance Corporation, 1931-1933*. Ames: Iowa State University Press. 1977. Pp. xi, 155. \$8.50.

The setting was the Herbert Hoover Presidential Library—a conference marking the thirty-first president's centennial, a session analyzing his administration's banking policies. The participants were celebrating his heroic effort to stabilize the credit system with voluntary cooperation instead of statism. In philosophy he had been on the side of the angels, appealing to banking leaders to eschew selfish individualism and build, in the autumn of 1931, a cooperative capitalist community in which the strong would save the weak without the intervention of bureaucratic Washington. But the big bankers had scorned his voluntary vehicle, the National Credit Corporation, and had demanded that the administration save credit institutions with the Reconstruction Finance Corporation capitalized by, not a banking pool, but the United States Treasury.

Hoover's cooperative community had been devastated; now, more than forty years later, historians were honoring his vision, even as they could concur with Richard Hofstadter—although without his sardonic tone, "Perhaps, after all, it was the spirit of the people that was not fundamentally sound." Nonsense, challenged one scholar. No president has the right to put a mythical community responsibility above a banker's two solitary obligations: to his stockholders and to his depositors. Hoover, the challenger implied, in his plea for voluntary cooperation had been guilty of errant idealism. The rebuke commanded respect; not only did it come from an academic specialist on banking, he also had been an assistant to the retired president and a researcher for Hoover's memoirs.

The incident suggests that there were two Hoovers, the one who espoused the cooperative and ephemeral NCC and the one who organized the

expedient RFC for its unintended two-decade career in state capitalism. Attentive to both, James S. Olson's focus on the RFC's origins and first year of operation should make his work one of the more important monographs on the Hoover presidency. But scholars will find it useful only if they are willing to put up with its uneven research, inconsistent interpretations, and frequent non sequiturs and banalities.

Olson's story of the origins of the RFC confirms earlier conclusions, based upon skimpier evidence, that it owed less to Hoover than it did to the demands of bankers for federal credit to save their own. Once the corporation came into existence, however, it became Hoover's responsibility. How did it perform? Here Olson is truly enlightening. It is a familiar tale that the big institutions received the big loans; in fact the RFC was not the "trickle down" agency assailed by its critics or the deposit insurance agency hoped for by supporters, because little credit was allowed by the big banks to circulate. The bankers used the loans either to liquidate their failing enterprises or else to purchase safe government securities whose productivity was left up to Washington. That compelled Hoover to endorse self-liquidating RFC loans to state governments for productive public works. Still, the RFC loans to both banks and states were obtained at interest rates that approached usury. Significantly, each state applicant was mandated to broaden its tax base through consumer levies. In effect, RFC loans to states for unemployment relief would be repaid by regressive taxes. The working poor would subsidize the jobless poor. Regressive taxes would be effective collateral for public works loans to states, thereby not depriving either banks or industry of investment capital through progressive taxes. No wonder its antagonists assailed all RFC loans as class action on behalf of the rich. They charged that Hoover, through his RFC policies, appeared insensitive to the needs of the poor; often appearances are not deceiving.

This brief book is inadequately executed partly because of an excessively narrow scope. The author recognizes that the RFC's prototype was the Wilsonian War Finance Corporation, but he has not studied it to any degree that would provide a more useful understanding of the RFC. Hoover's initial reluctance to employ the RFC for public works is explained by his fear of an unbalanced budget, but Olson neglects an exploration of Hoover's fiscal policies. Instead, he elucidates considerably upon Hoover's political philosophy which, as I suggested earlier, has no meaning even for certain disciples.

Finally, despite his assignment of responsibility to Hoover for the RFC's first year of "failure" to generate any semblance of recovery, Olson's last

sentence deplores the fact that Americans have "never really understood him nor, for that matter, ever really appreciated him." This is Hofstadter rephrased, a sign that Hoover may have been an historical failure but is a theoretical success with historians.

JORDAN A. SCHWARZ  
*Northern Illinois University*

LYLE W. DORSETT. *Franklin D. Roosevelt and the City Bosses*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. x, 134. Cloth \$8.95, paper \$4.95.

This book brings together under one cover the stories of FDR's relationships with seven urban politicians in six cities. For anyone familiar with the individual memoirs and secondary works concerning bosses James M. Curley, Ed Crump, Ed Flynn, Tom Pendergast, Ed Kelly, Frank Hague and Mayor Fiorello LaGuardia, Lyle W. Dorsett's account offers little new; however, as a compilation, it has some use. One of the author's main themes—that the New Deal did not destroy the bosses and their machines, but in several cities strengthened them—has already become an accepted interpretation in the urban history texts written during the 1970s. Had this book been published several years earlier, its contribution might have been fresher, despite the volume's traditional approach to the study of urban politics.

The author's second theme, Roosevelt's self interest in dealing with the bosses, is treated with a surprising element of oversimplification. New Deal scholars have been well aware that FDR was not pristine pure in his political activities, yet much of the book is given over to a one-sided indictment of the president's disloyalty and deceit. Vengeful FDR made certain that Curley went to jail on mail fraud charges. Callous FDR never followed through on his promise to make LaGuardia a brigadier general, and "this ego devastating blow took much of the fight out of LaGuardia," who decided not to seek reelection in 1945, went into retirement, and died two years later (p. 63). Was there really cause and effect as implied? Ungrateful FDR did not support Ed Flynn in his fight to win confirmation of his appointment as minister to Australia, since Roosevelt "made a habit of deserting his friends and allies if they stood in the way of his own ambition" (p. 68). Amoral Roosevelt shunned Tom Pendergast not because he was morally outraged at the boss's corruption, but because he believed Pendergast had become a loser (p. 81). Uncouth Frank Hague may have personally disgusted patrician FDR, but because he controlled so many New Jersey votes, the president tried to



stay friendly, even if at a distance. Many Americans may have idolized charismatic FDR as a warm and sincere human being, "but when one examines his heartless, disloyal, and ultimately ruinous treatment of individuals he pretended to befriend, it makes one stop and wonder if he ever did *anything* without considering his own political self-interest" (p. 49).

While we might wonder about this, urban scholars interested in ranging beyond simple Manichaean divisions would desire that this book had considered more deeply questions concerning the structure and functions of urban politics related to the nationalizing forces of the 1930s. The inclusion of Fiorello LaGuardia in a book about urban bosses raised the issue of the similarities and differences between bosses and reformers, which Dorsett more effectively dealt with in a 1972 article. The reader is correctly informed that the machines' mass base was broader than the working class and the poor, but a study of FDR's ties to the grass-roots urban ethnic communities both through and around the boss system would not have been out of order.

Many years ago, the Tammany politician George Washington Plunkitt asserted that the bosses would not "get the right sort of epitaphs" until they were gone. The president and the bosses considered in this book are all long gone but still await "the right sort of epitaphs."

BRUCE M. STAVE  
University of Connecticut,  
Storrs

CHARLES H. TROUT. *Boston, the Great Depression, and the New Deal*. (Urban Life in America.) New York: Oxford University Press. 1977. Pp. xx, 401. \$15.95.

This is the second of the books in Richard Wade's "Urban Life in America" series that contend with that great crossroads of local-federal political relationships: the New Deal. In 1975 Mark Gelfand's judicious volume, *A Nation of Cities*, analyzed Washington's response to urban crises from Roosevelt's arrival in 1933 until the establishment of the Department of Housing and Urban Development in 1965. Much of what occurred during that span, Gelfand admits, was talk—an uncertain buildup toward the ideal urban society Americans still cannot clearly envision.

Charles H. Trout's massive compression of evidence about America's original City on a Hill helps show how some urban recipients of Washington's early attempts at guidance actually fared. In so doing Trout carefully emphasizes the precarious balances that were the only practical alternatives to chaos. Trout thus connects with the

debate of recent years over the degree to which the New Deal should be considered for its local as against its national implications.

Trout has extended the argument with a comprehensive examination of official data, memoirs, journalistic reflections of the era, and secondary literature. Not one of those "cold statistical Christs" Boston's own John Boyle O'Reilly warned against, Trout shows uncommon ability at narrative synthesis, drawing together a vast amount of detail about political maneuvers, banking and finance, union activity, ethnicity, and relief efforts into a persuasive story line. The central figure is Mayor James Michael Curley. Trout perceives that Curley was both the dominant practitioner of Boston ethnic politics and the first to appreciate the rising star of Franklin Roosevelt. Purposely estranging himself from the small-time clan politicians who clung futilely to Al Smith after 1928, without ever allowing the national interest to eclipse his own, Curley personified the trials of an American city in transition.

Accordingly, Trout is alive both to the color of Boston politics and the broad matrix of substantive change. His flair for setting scenes, with their particular human dramas, enables Trout to compress a great deal of data into his account without losing the texture of human experience. He enables us to see in full context the gall and humor of Curley and company, the smugness of their State Street and Beacon Street antagonists, the insistence of Cardinal O'Connell on the volunteerism that rose higher in Boston than elsewhere—all amidst the grim struggle of the working class to salvage some of its industrial base. Trout's approach is especially well suited to conveying the sense of Boston as a tight little island. We can sympathize with Harry Hopkins' decision to make the city one of the few places where federal relief would have to be under direct federal control; we sympathize as well with Hopkins' agents, who were urged to "take some bulletproof vests along."

Trout steadily maintains that Boston struck a number of saving balances during the Depression. He resists the suggestion of Martha Gellhorn, whose melodramatic reports to Harry Hopkins from all over the country did so much to raise concern over the plight of the dispossessed, that the representative Bostonian was in "a state of semi-collapse...." He resists as well the conclusion of Edwin O'Connor in *The Last Hurrah* that the New Deal sounded taps for the big city bosses by replacing patronage with federal welfare. Neither a collapse of the will nor its counterpart, New Deal rescue, characterized Boston's response to the Depression. Rather, innovation deferred to a persistent social and economic style. The Hub's financial structure, AFL union base, conservative



Catholicism, and ward politics rode out the storm. Interestingly enough, Trout uses as his prime example of persistence the same undertaking Gelfand points to as the touchstone of federal policy toward the city: public housing. Though Boston was chosen as a likely model for low-cost housing, Trout explains, its phalanx of political and contracting interests prevented any such unprofitable building. At the same time the Civil Works Administration (CWA) pressed ahead with construction so much under the patronage wing of politico Charles Hurley that its operation became known as "Charlie's Workers Administration."

Still, Boston, as everywhere, had to undergo changes during the great upheaval of the Depression. Trout indicates, though not with explicit awareness, that each of the two major areas of change developed reciprocal balances. First, although Massachusetts slipped more than any other New England state in the region's drop in its labor force engaged in manufacturing, the formation of union locals in the city increased at an unprecedented rate. In that parlous state Boston's unions were so inhibited about risking social change that Trout uses their timidity to help explain why the New Deal had seemed overcautious to some: "If a supposedly indispensable part of the Roosevelt coalition held back, there could be no constituency for sweeping social change" (p. 199).

The second major change was in neighborhood structure. Shifting fortunes caused unusual movement and broke up some long-standing enclaves. Yet that very mobility, and the hard times behind it, also increased ethnic consciousness and warfare. Nevertheless, as Trout points out, Father Coughlin's "crusade" made scant electoral headway in Boston. Governing all was the balance between the old, persistent, cultural style and a new fealty to Franklin Roosevelt. Whatever went wrong when New Deal programs entered the city, Trout tells us, Roosevelt was seldom blamed. He was the inspirer and savior—the polestar toward which Boston's long-repressed Irish majority could chart its eventual rise to the reflected glory of John Kennedy.

ALAN LAWSON  
Boston College

DAVID DUBINSKY and A. H. RASKIN. *David Dubinsky: A Life with Labor*. New York: Simon and Schuster. 1977. Pp. 351. \$9.95.

When David Dubinsky assumed the presidency of the International Ladies Garment Workers Union (I.L.G.W.U.) in 1932, he inherited a shattered organization which faced bankruptcy. Uncertain as

to whether he would serve as leader or as undertaker Dubinsky nevertheless accepted the challenge. "I was never short on *chutzpah*," he commented. On his retirement from office in 1966, the I.L.G.W.U. ranked among the most prosperous, innovative, and powerful unions in the country. In this autobiography Dubinsky tells his own story in a series of tape-recorded interviews arranged and edited by *New York Times* labor expert A. H. Raskin.

In large measure the I.L.G.W.U.'s resurgence came as the result of Dubinsky's ability to convince employers that the union acted as a constructive force for them as well as for workers. In the past, companies which had tried to maintain decent wages and conditions had found it hard to compete with sweatshops. Strict enforcement of union standards mitigated cut-throat competition and produced stability for the employer and security for the employee. Twenty-five years without a major strike also enabled the I.L.G.W.U. to pioneer in pension and welfare programs, health centers, adult education, the establishment of a vacation resort, and the construction of a gigantic housing development.

Under Dubinsky's leadership the I.L.G.W.U. exerted an important influence on the national labor movement and on New York City politics. Within the A.F. of L. Dubinsky supported efforts by John L. Lewis to organize new unions along industrial lines although he refused to join the C.I.O. when it separated from the Federation. Dubinsky also contributed to the fight against labor racketeering which led the A.F. of L. to expel the International Longshoremen's Association and the International Brotherhood of Teamsters. In New York City, the I.L.G.W.U. created the Liberal Party to act as a balance of power between the Republicans and the Democrats.

Dubinsky's autobiography makes an informative contribution to American labor history; it can serve as a useful source for further studies. At the same time, as A. H. Raskin notes, "This is not a balanced story, nor does it pretend to be. You will search it in vain for any acknowledgment of error, any sense of important failure, any questioning either of goals or of their fulfillment" (p. 12). It is history seen through the eyes of one of America's most humane, shrewd, and inventive labor leaders, who demonstrates again that he was "never short on *chutzpah*."

GRAHAM ADAMS, JR.  
Mount Allison University

VERA BUCH WEISBORD. *A Radical Life*. Bloomington: Indiana University Press. 1977. Pp. xviii, 330. \$15.00.

Vera Buch Weisbord participated in several signal episodes in the American labor and radical movements and recognizes a duty to describe them with detachment, although from her own vantage point. She experienced the range of occupational crises peculiar to women, and by the time (at age eighty) she recounted them, she could in the main view this aspect of her life in the light of current feminist ideas foreign to her during her active years.

Weisbord's autobiography examines her life on three levels. The first is her public life, dominated by her connection with left-wing socialism and membership in the Workers (Communist) Party, both in its underground and open-existence years, and by her subsequent break with the party. She was active in the Passaic, New Jersey textile-workers' strike of 1926 and in the Pennsylvania coalfields. While organizing the National Textile Workers Union in Gastonia, North Carolina, she faced her greatest hazard, a bitter, protracted strike and jail. She provides keen characterizations of notable personalities in the socialist and Communist movements with whom she had contact. Also, she accuses the party of failing to provide needed support in strikes and for dragging its feet or failing entirely in providing defense for jailed organizers and strikers.

A second level concerns the childhood, college, and labor-movement relationships. These were chiefly with women: Irish, Italian, and Jewish youngsters in her neighborhood who provided her first lessons in ethnicity; sister patients in tuberculosis sanatoria, one of whom introduced her to the Marxist movement; partners on the job or in awesome and unequal struggle on the picket line. The author describes warm, touching, even poignant encounters.

The last level is the intimate side of her life. As her mother martyred herself to children and a self-absorbed, inept husband given to rages and incapable of providing for the family, so young Vera Buch attached herself to egocentric Albert Weisbord, erratically brilliant, fitfully employed, who directed the Passaic strike. He also led the tiny, ephemeral Communist League of Struggle (1930-35), for which Vera Weisbord edited *The Class Struggle* and about which she had a presentiment: "This cannot succeed" (p. 302). In the duo she was ever the helpmeet. Early on, offered a party post requiring her relocation, she allowed her petulant husband to dictate her refusal, after which she usually followed him on his party assignments. She resigned herself to labor-union work while he, "the revolutionary," traveled widely abroad. Because he decreed no children for their union, she underwent a primitively administered abortion and paid later with painful illness. Because he dismissed the operation as a woman's affair, she

endured the ordeal alone. And uncomplainingly (with most radical women) she accepted second class status in the party.

The student of labor history is well advised to consult Vera Weisbord's chapters on Passaic and Gastonia for the dimension they add to existing accounts and for the corrections she claims to give. An excellent introduction by Paul Buhle and Mari Jo Buhle provides the scenario and an analysis. Weisbord herself explains obscure points in six short appendixes. The narrative halts in 1935 with the remaining forty years telescoped into an epilogue, so the book gives the story of only half a radical life. But as far it takes us it is fine autobiography.

CONSTANCE ASHTON MYERS  
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Aiken

CHALMERS M. ROBERTS. *The Washington Post: The First 100 Years*. Boston: Houghton Mifflin Company. 1977. Pp. xiii, 495. \$15.95.

In 1933 the *Washington Post* was "journalistically comatose" and financially bankrupt. Had it not been purchased by Eugene Meyer, who was wealthy enough to support a newspaper that continually lost money, the *Post* probably would have followed many another metropolitan daily into the oblivion of merger with a competitor.

The paper had known better times. Under the able editorship of its founder, Stilson Hutchins, the *Post* quickly became known as the best newspaper in Washington, despite its avowedly Democratic editorial slant. Under publishers John McLean (1905-16) and his playboy son Ned (1916-33), however, the paper steadily deteriorated. Poor editorial guidance was coupled with a growing sensationalism of the Hearst variety; countless times the *Post* failed to send reporters to important events, relying instead upon AP dispatches. Meyer, the publisher from 1933 to 1945, took an active interest in the *Post*; he hired new management and helped bring the paper out of the doldrums. It was under the leadership of his brilliant (though erratic) son-in-law, Philip Graham, however, that the *Post* made the great leap forward, finally surpassing its rival, the *Star*, and becoming the leading newspaper in Washington by the end of the 1950s. Upon Graham's death in 1963, his wife Katharine took the helm. By the 1970s the paper had gained fame from Watergate and had attained a national reputation second only, perhaps, to that of the *New York Times*.

Roberts, for many years a *Post* reporter, has written a sprightly and informative account of the paper's first century. This is no powder-puff treat-

ment, however, and in fact the author is a model of objectivity in analyzing the personalities and policies of his newspaper. Prior to 1946 the *Post* displayed little of the liberalism for which it is now renowned. It helped foment the post-World War I Red Scare, supported the reactionary politics of the Republican Party in the 1920s (editor Ned McLean was even involved in the Teapot Dome scandal), and remained hostile to much of the New Deal. The paper either ignored blacks entirely or dealt with them in a stereotyped manner, and the *Post's* inflammatory reporting in 1919 undoubtedly contributed to the Washington race riot of that year.

With Philip Graham the *Post* began changing, but for many years the paper's liberalism was mostly in the areas of civil rights and civil liberties, not economic policy. In analyzing foreign relations the editors followed a firm Cold War line until well into the 1960s; the *Post* was one of the last major dailies to break with Lyndon Johnson over the Vietnam War.

Political news coverage plays a major part in Roberts' story. He also devotes considerable attention, however, to the paper's handling of society news, sports, the black community, and social problems in general in Washington. Numerous anecdotes enliven the text, and only occasionally does the author succumb to the temptation to include unnecessary trivia. The book is obviously well researched, although footnotes would have been helpful.

This volume will be a delight to the general reader. Historians may well question, however, whether organizing the chronology around the owners is the best approach. Less attention to the owners and more (especially during the paper's early years) to the professionalization of journalism, the technology of publishing, and the relationship between social trends and changing newspaper content might have been more appropriate and enlightening. These criticisms aside, *The Washington Post* must rank as one of the best newspaper "biographies" ever written, and it should find wide usage in courses dealing with the history of journalism.

KENNETH L. KUSMER  
Temple University

BENNETT CERF. *At Random: The Reminiscences of Bennett Cerf*. New York: Random House. 1977. Pp. ix, 306. \$12.95.

Book publishing was his line, and in *At Random* we have Bennett Cerf's posthumous story. At one level, the book is simply a grab bag of highly readable, often funny reminiscences about the rich

and/or famous: FDR dropping Cerf from the White House Christmas card list after Random House remaindered a volume of his speeches; Douglas MacArthur, in his incarnation as board chairman at Remington Rand, a corporate sponsor of "What's My Line?," vetoing a 1956 "Mystery Guest" appearance by Harry Truman. More substantively, it fleshes out and personalizes the account John Tebbel, Charles Madison, and others have given us of the process by which a genteel profession dominated by a coterie of elderly WASP conservatives was transformed in the 1920s by a heterogeneous group of brash young newcomers. Cerf offers a unique perspective not only on this process but also on the censorship battle, the rise of book clubs and paperbacks, the takeover by conglomerates in the 1960s, and other facets of publishing history. If the profession's technological, economic, and even literary aspects are only tangentially touched upon in this light memoir, one learns much of its personal, social, and public-relations side.

About Cerf himself we learn mainly externals: the boyhood in then-Jewish Harlem; the Columbia years; the 125,000-dollar bequest from a merchant grandfather; the apprenticeship with the legendary Horace Liveright; the 1925 purchase (with Donald Klopfer) of Liveright's "Modern Library" series, which launched Cerf's independent career; and the subsequent Midas-like success of Random House, culminating in its 1965 sale to RCA for forty million dollars. Cerf's special talents were dealing with authors, generating new publishing and publicity ideas, and recruiting skilled editors and executives. He emerges as a likeable, uncomplicated man with a liberal, secular, and hedonistic bent; a firm belief in the benevolence of a free-enterprise system tempered by goodwill and "decency"; and an enormous capacity for bonhomie. (Theodore Dreiser is almost unique in having elicited his intense dislike.) The smiling, wisecracking, avuncular figure on television seems to have been the essential Cerf.

*At Random* also reveals a love of publicity so vast as to be almost endearing: an innocent quest for attention avidly and endlessly pursued via radio, television, billboards, newspaper columns, and public appearances. Bennett Cerf perfectly exemplified the contemporary type delineated in Daniel Boorstin's *The Image*, for whom celebrityhood becomes a way of life. (In Oxford, Mississippi for William Faulkner's funeral, he was treated with thinly veiled hostility until the family recognized him as a famous TV personality, after which their friendliness knew no bounds.) But this dimension of Cerf's career ought not obscure his genuine and lasting significance as a towering figure in the world of twentieth-century book pub-

lishing. Once again we have reason to be grateful to the Columbia Oral History Project, for whom Cerf recorded the twenty-one interviews from which his widow, Phyllis Cerf Wagner, and Random House editor Albert Erskine shaped this book.

PAUL BOYER  
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Amherst

JAMES R. LEUTZE. *Bargaining for Supremacy: Anglo-American Naval Collaboration, 1937-1941*. Chapel Hill: University of North Carolina Press. 1977. Pp. 328. \$17.95.

This book traces various Anglo-American diplomatic and naval negotiations from the summer of 1937 through the ABC Conference of 29 January—29 March 1941, when the Americans finally prevailed upon the British to accept their “Atlantic First” strategy if the United States became involved in war with Germany, Japan, or both. James R. Leutze properly records that American and British officials regarded one another with suspicion and sometimes hostility, were reluctant to share technical information or to make political commitments that might be construed as pulling the others’ chestnuts out of the fire, and bargained for technical or political advantage. The United States usually won the major concessions because of its superior resources and Britain’s deteriorating military and financial situation.

This analysis will not surprise scholars familiar with the era, Anglo-American relations, or the nature of nation states, international politics, and bureaucratic/institutional rivalries—the traditional rhetoric of Pilgrim’s Club speeches and the post-World War II haze of the “special relationship” notwithstanding. Leutze seeks, however, to give Anglo-American negotiations and the ABC agreement special significance. He argues that the American decisions to build a navy second to none, to assume the primary role in the Atlantic/European theater, and to fight a defensive war in the Pacific while not reinforcing the British at Singapore meant that the United States sought to replace the British as the major power in the Western world and to assume supremacy in the wartime coalition that would negotiate the political and economic parameters of the postwar world.

There is merit in this thesis which, as Leutze suggests, should remind historians that, for better or worse, the United States was inclined to bargain as firmly with the imperial British as with Soviet Russia. But two questions remain unanswered. Was there a real alternative to the American “Atlantic First” strategy? Was it designed to achieve

supremacy over the British or as the best means to defeat Nazi Germany, which was perceived as the primary enemy? As General George Marshall said, “If we lose in the Atlantic we lose everywhere” (p. 166). Moreover, Britain’s declining role in international affairs resulted from large and long-term political and economic factors, not American military strategy and tactics, which, contrary to Leutze’s implications, Churchill influenced after 1941, as witness his blocking a cross-Channel invasion in 1942. Further, while reading Foreign Office memoranda and marginalia is instructive and literarily engaging, one must remember that generally the British demonstrated immense Victorian conceit in dealing with their American counterparts, and leaders of both the Chamberlain and Churchill stripe clung to the dubious idea that air and naval pressure alone would defeat Germany economically.

Overall, this book, which is nicely written although occasionally given to excessive detail and flamboyant phrases, affords an interesting narrative and insights into Anglo-American negotiations, but it is no substitute for a comprehensive analysis of Anglo-American relations in the 1930s and their relation to the coming of global war in 1941.

ARNOLD A. OFFNER  
Boston University

A. RUSSELL BUCHANAN. *Black Americans in World War II*. Santa Barbara, Calif.: Clio Books. 1977. Pp. ix, 148. \$14.95.

In this slender volume historians familiar with blacks and the Second World War will find nothing new and much omitted. For one thing, A. Russell Buchanan’s *Black Americans in World War II* is the third study in as many years on the subject. In 1975 Lee Finkle published *Forum for Protest: The Black Press during World War II*, and in 1976 Neil A. Wynn came out with *The Afro-American and the Second World War*. To some extent, all three are complementary, although Buchanan’s study suffers in comparison with Wynn’s, which is not only based on a much richer variety of primary and secondary sources but is also more comprehensive chronologically as well as topically. To Buchanan’s credit, he fulfills most of his modest claims, and his conclusions are restrained as to the war’s impact on American blacks.

Buchanan focuses on such familiar topics as the March on Washington Movement of 1941, black migration and employment (including a brief chapter on Negro women), blacks in the military (based extensively on Ulysses Lee’s *The Employment of Negro Troops*), race riots, and the “Double V”

campaign that emphasized victory against racism both at home and abroad. He concludes that as a result of the war the "question of the Negro's place in society ceased to be mainly a southern concern and became a national issue" (p. 131).

Buchanan writes clearly. The absence of conclusions to some chapters, however, obscures his main contentions. There are also several nagging errors, and it is distressing to find Will Alexander, a prominent white southern liberal, identified on two separate pages as a well-known Negro leader, which may suggest something about the author's research. Among manuscript collections, Buchanan examined only those of the NAACP and the National Urban League. He virtually ignored the black press, although he has a few isolated references to the *Pittsburgh Courier* and the *Norfolk Journal and Globe* [sic]. The "Selected Bibliography" is much too selective. Of the twenty-seven articles listed, only five appeared in magazines or journals other than the *Crisis* and *Opportunity*, and only one of the twenty-seven is a scholarly, secondary account published after the war. Missing are the significant essays of Richard M. Dalfume, Harvard Sitkoff, Lee Finkle, and Neil A. Wynn, among others.

Still, Buchanan's study may have its uses. On the back cover of the book, it is stated that it "is intended as a reader for courses in Afro-American history, ethnic history, and sociology." This is a commendable goal, for students unfamiliar with blacks and the war will undoubtedly profit from this volume. But the publisher's price of \$14.95 may frustrate even this objective.

RICHARD T. RUETTEN  
San Diego State University

CLARK M. EICHELBERGER. *Organizing for Peace: A Personal History of the Founding of the United Nations*. New York: Harper and Row. 1977. Pp. xiii, 317. \$15.00.

Efforts to establish an effective union of nations have been increasingly commonplace in the twentieth century, yet only a few participants in that crusade have described their roles. Clark Eichelberger was one of the most dedicated of these internationalists, a man who became a convert to the League of Nations ideal while a soldier in France in 1918. In the 1920s he established himself as a speaker and director of the Chicago office of the League of Nations Non-Partisan Association, and he rose because of organizational talents and personality to head the League of Nations and United Nations Associations. He became a confidant of world and national leaders and played a crucial role between 1938 and 1945 in attempts to

revise the neutrality acts, promote aid to the Allies, and plan for a new world organization.

His book is difficult to classify. Eichelberger calls it "a personal history" written "from the perspective of his own experiences," yet he says it is not autobiographical. The work is episodic, with 65 pages for 1920-37, 89 for 1938-40, 133 for 1941-45, and virtually nothing on postwar developments. It also shifts continuously from a general history of times and developments to paragraphs describing Eichelberger's involvement with events and persons. While it contains some new insights and perspectives, these are disappointing in number, and it should be read as a supplement to Walter Johnson's *The Battle Against Isolation*, Robert A. Divine's *Second Chance*, Dorothy Robins' *Experiment in Democracy*, and Harold Josephson's biography of James T. Shotwell.

Of special interest are Eichelberger's descriptions of the strategy to shift some League of Nations operations to the United States after 1939, his comments on regionalism as the basis for an international organization, his account of a trip to England in September 1941, and his remarks on the Welles-Hull rift, atomic energy planning, human rights, and the nature of UN membership. His detailed presentation of conversations with Franklin Roosevelt are revealing but should be read with Divine's study in mind. Eichelberger admired Roosevelt and is rarely critical. Readers hoping for detailed information on the groups and individuals Eichelberger worked with will be disappointed. There is little on ideological differences or on the internal operations of the League of Nations or United Nations Associations. Their history must still be written.

Eichelberger is surprisingly modest regarding his role in affairs. History has shown the correctness of his position in insisting that the United States assume a greater responsibility to maintain peace in the 1930s, in his promotion of the idea of collective security, and in his persistent endeavors to erect a new international system. The most valuable feature of this book lies in his capacity to convey the enthusiasm and faith of exponents of the League of Nations and United Nations and to reveal that dedication to ideals which prompted his tireless quest for a better world.

WARREN F. KUEHL  
University of Akron

JOHN GIMBEL. *The Origins of the Marshall Plan*. Stanford: Stanford University Press. 1976. Pp. xii, 344. \$15.00.

In this ambitious work John Gimbel argues that those who see the Marshall Plan arising as an



integral part of the containment policy are mistaken in emphasis, as are those who believe the plan was shaped primarily by an American desire to foster an open world.

Gimbel makes a number of major points. Although Russia was a skillful bargainer on matters relating to Berlin and Germany, the Soviets were not unreasonable in their attitudes. It was France that proved recalcitrant on establishing central zonal administration, on reparations, and on the level of industrial production to be permitted in Germany. France continually resisted fulfilling agreements the Big Three reached at Potsdam, hoping to force major boundary adjustments with Germany and to structure the German economy so that Germany would not be able to reassert hegemony.

Secretaries of State Byrnes and Marshall, together with most of their subordinates, were very sensitive to French desires. The State Department, in charge of policy planning for the American zone in Germany, envisioned France as the centerpiece of a reconstructed Europe and therefore a power whose concerns must be carefully weighed. The department was also responsive to the economic needs of other European powers who felt they had claims upon German resources.

It was the army, unwillingly appointed as zonal administrator by President Truman, which found French policies unacceptable on central administration, level-of-industry quotas, and reparations. Army administrators feared congressional wrath over army budgets needed to run a depleted and economically repressed Germany. General Lucius Clay, chief advocate for the army viewpoint, constantly pushed for a more rapid rehabilitation of German industry than the State Department or France thought wise.

French policy was not the only source of contention between the State Department and the army. The British also provoked major tensions with their desire to nationalize major industries in the British zone and by their unilateral decision to raise level-of-industry quotas. In the State Department/army struggle over policy for Germany, Gimbel argues, one finds the chief impetus shaping that series of political compromises and economic arrangements we know as the Marshall Plan.

Anti-Soviet rhetoric was used by the Truman administration to sell the Marshall Plan to Congress and the public. A brilliant public relations success, this tactic had the virtue of minimizing American difficulties with France and Great Britain as well as hiding the Army/State Department struggle over German and Central European policy.

Despite the author's denigration of other inter-

pretations, doubts remain. Were perceptions of Russia as peripheral to the plan as the author suggests? Were State Department officials as well disposed toward France as Gimbel insists? Was the development of export markets really of so little concern to the United States?

Gimbel, in his insightful analysis on bureaucratic policy battles over Germany, has greatly enriched our understanding of the context in which the Marshall Plan developed. But his controversial and stimulating book is unlikely to stand as sufficient in itself to explain the origins of the Marshall Plan.

MARVIN R. ZAHNISER  
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ROBERT J. DONOVAN. *Conflict and Crisis: The Presidency of Harry S Truman, 1945-1948*. New York: W. W. Norton. 1977. Pp. xvii, 473. \$12.95.

This is a major addition to the literature on the Truman period. The author, a distinguished journalist, combines the skills and methods of his profession with the methods and accomplishments of ours. Using traditional historical sources, including the now rich holdings of the Truman Library, as well as interviews, he also exploits the full range of writings by historians on his subject and generously acknowledges his debts "to a generation of scholars . . . without whose labors on different aspects of postwar history the story of the Truman presidency could not possibly be told" (p. xix).

The contrast between this book and what had been the best effort by a journalist to survey the subject testifies to both the progress of the field and the industry of the author. Cabell Phillips in *The Truman Presidency*, published in 1966, covered the entire period in less than four hundred pages. Robert J. Donovan employs well over four hundred and explores only the first term, leaving the second to another volume. Phillips used the scholarly literature in a highly selective way—ignoring the revisionism that had already emerged, complained about the inadequacies of the materials available in the Truman Library, and relied chiefly on interviews for his contributions. Donovan conducted many more interviews and drew also on the work of oral historians.

The chief shortcoming of Donovan's book is the absence of a large controlling theme. He brings together a vast amount of information, is at his best when describing episodes and people, and adds to our knowledge and understanding of them. He does not, however, debate with the scholars about their interpretations, supply a bold thesis to which the details are subordinated, or address the questions concerning Truman and the Russians

that are fundamental to appraisals of his presidential career.

Nevertheless, Donovan does not neglect the historian's duty to interpret. While agreeing with those who argue that Truman bungled badly at times and contributed to the rise of irrational anti-Communism, he presents a predominantly positive appraisal of the man. He is impressed by the contrast between what was expected and what Truman accomplished and refuses to give all credit to advisers. His list of accomplishments resembles Truman's own and those of liberal historians; his explanation of Truman's failures in domestic affairs stresses the difficulties in the situation, and his interpretation of the man denies that he was swayed only by political considerations and lacked commitments to liberal and humanitarian principles. He stresses continuity rather than discontinuity when dealing with the switch from Roosevelt to Truman, believes that the United States contributed to the outbreak of the Cold War but does not blame Truman for it, and sees political factors at work in the decision to use the atomic bomb but does not emphasize them and does not criticize Truman for the decision. And the book's most significant essay in interpretation supplies an explanation for Truman's behavior in foreign affairs that, while not ignoring capitalism and capitalists, emphasizes the influence of the history of the 1930s.

RICHARD S. KIRKENDALL  
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DOUGLAS KINNARD. *President Eisenhower and Strategy Management: A Study in Defense Politics*. Lexington: University Press of Kentucky. 1977. Pp. xi, 169. \$13.75.

In *President Eisenhower and Strategy Management*, Douglas Kinnard provides a useful, sensible, and concise study of Dwight D. Eisenhower's performance as an originator and manager of defense policy. Kinnard musters evidence, considerable in volume given the comparative brevity of the book, to portray Eisenhower as very much his own man in the formation and pursuit of strategic policy rather than a frontman for his aides such as John Foster Dulles and George Humphrey. Kinnard further argues, in a valuable insight, that Eisenhower's establishment of staff procedures did not result in the surrender of defense policy management to his subordinates. Instead, procedures were so designed as to permit the final coordination of diverse aspects of security policy only in the White House and particularly in the Oval Office. Preferring to be a "skilled practitioner of closed

politics" (p. 136), Kinnard's Eisenhower is no man's fool or pawn.

Kinnard accurately associates Eisenhower's views on military strategy with the president's belief in the prerequisite importance of economic and fiscal soundness. Evidently, Eisenhower failed to persuade the various service chiefs of the mutuality of economic and military security; and, Kinnard concludes, "he clearly recognized that his chief adversaries were the service chiefs . . ." (p. 64). Partly because of this, Eisenhower, like Truman before him, attempted to use reorganization of the Department of Defense (in 1953 and 1958) as a means of gaining greater control for the White House over the military budget, the roles and missions of the services, and consequently over military policy.

Given the significance of the Eisenhower administration's reasons for seeking reorganization of the military, one might have expected somewhat more detailed treatment of both the 1953 and 1958 efforts. But perhaps the major interpretative flaw in the book is Kinnard's subscription to the thesis that the New Look was an extraordinary innovation. Distinctive as the New Look was, it bore a strong kinship to many ideas that had developed in the Truman years, as greater attention to documents from Truman's administration would have shown. That is not to ask Kinnard to have written a different book. Rather, the thesis that Eisenhower's efforts constituted a major strategic innovation requires a specific and debatable interpretation of Truman's defense program before Korea. Nor should one assume that even Truman would have sought a return to the exact strategy prevailing in the years before the war (p. 126). If anything, Kinnard's remarks about Eisenhower's goals and means—in the use of reorganization of the defense establishment to exert greater control over military policy, for example—would apply with considerable accuracy to Truman as well. The odds just as much would have favored Truman's seeking considerable change in the system about which he so often expressed reservations. Although this interpretation of the transitional period 1953–54 tends somewhat to exaggerate the novelty of Eisenhower's policies, Kinnard's major argument remains persuasive.

Since considerable material from the Eisenhower years remains to be made available to researchers, Kinnard will not likely have the last word on many of the specific problems to which he refers only briefly in this book. Nonetheless, he provides a useful contribution to the continuing definition of Eisenhower's performance as president and as commander-in-chief.

DONALD J. MROZEK  
Kansas State University

ANTHONY CAVE BROWN and CHARLES B. MACDONALD, editors. *The Secret History of the Atomic Bomb*. New York: Dial Press/James Wade. 1977. Pp. xxi, 582. Cloth \$16.95, paper \$5.95.

ANTHONY CAVE BROWN, editor. *Drop Shot: The U.S. Plan for War with the Soviet Union in 1957*. New York: Dial Press/James Wade. 1978. Pp. vi, 330. \$12.95.

These two volumes, edited by established writers of good repute, represent a new dimension in the commercial exploitation of recently declassified government documents. Although the contents of these books will be of some interest to scholars in the fields of history of technology and national security policy, the drab official prose will leave the general reader feeling that he has been gulled by vivid dust jackets promising the disclosure of new and exciting Cold War secrets.

*The Secret History of the Atomic Bomb* contains almost six hundred pages of fine print selected from the thirty-five-volume official *Manhattan Engineer District History* of the development of the atomic bomb. To fill the gaps left by still-classified sections of the official history concerning the gaseous diffusion process and the actual building of the bomb, the editors have included portions of two declassified articles which cover related topics. As their first section, they include the bulk of the Smyth Report, prepared as a general account of the scientific development of the bomb and published shortly after the first use of the bomb against Japan. This report, written by H. D. Smyth of Princeton University, excluded both technical information not already known by competent scientists and any details of the construction of the bomb. Its omissions notwithstanding, the Smyth Report is the most interesting part of the book. Most of the remaining 450 pages contain turgid official summaries of auxiliary efforts such as the heavy water and electromagnetic projects or descriptions of activities at Oak Ridge, Hanford, and Los Alamos. Some material of interest can be found in the final section, a report by the Manhattan Project Atomic-Bomb Investigating Group which describes and analyzes the effects of the two different types of bombs on people and buildings in Hiroshima and Nagasaki.

*Dropshot* is potentially a more important publication for general readers and scholars who are unfamiliar with strategic planning documents. Drafted by a committee of the Joint Chiefs of Staff in 1949 when Soviet power and ambitions appeared to be growing rapidly, it presents the assumptions, objectives, and strategic requirements for a United States response to a general Soviet attack projected to occur early in 1957. Assuming that the ultimate Russian goal was world domina-

tion, the JCS planners expected the United States to use every possible means of resistance, including atomic weapons and "an extensive psychological-warfare campaign whose basic objective will be to destroy the support accorded by the people of the USSR and her satellites to their present systems of government and to fasten a realization among the peoples of the USSR that the overthrow of the Politburo is an attainable reality" (p. 62). *Dropshot* provides some of the flavor of military planning as the intensity of the Cold War increased. But the repetitions within the published sections of the plan and the failure of the drafting committee to coordinate objectives with the resources likely to be available will leave many readers confused.

Unfortunately, *Dropshot's* value remains unrealized because of inadequate editorial scholarship. Anthony Cave Brown does not tell us who wrote this 1949 plan or where it stands in the evolution of United States strategic policy toward the Soviet Union. He implies that it was an operational war plan which through its presentation of America's military weakness persuaded Washington officials to begin a massive rearmament program. Had Brown checked into materials available in the same files as this plan, he could have learned that "Dropshot" was drafted by an ad hoc group from the Joint Strategic Plans Committee of the Joint Chiefs of Staff, that it was only a study and never became an operational war plan, and that as "a requirements plan" its purpose was to work backwards from the national objectives set forth in NSC 20/4 to establish the force and logistical requirements needed to fulfill the objectives already adopted by the National Security Council. Through numerous revisions, the drafting committee found that the goals of NSC 20/4 were so broad and imprecise that it was impossible to base a plan on them. In the meantime, this planning exercise was overtaken by events such as the Soviet atomic detonation of August 1949, the more ambitious objectives of NSC 68, and, most importantly, the outbreak of the Korean War.

The principal criticism of these two books is that the publisher and the editors, by their titles, layouts, and publicity, promise a degree of new and valuable information which is not delivered in these declassified documents. The editors do not provide sufficient introductory and explanatory material to indicate the significance of what they present. In some cases, especially in *Dropshot*, the editorial apparatus is positively misleading. We need responsible official histories of major organizations such as the Manhattan Engineer District and published strategic war-plans with appropriate explanations of their nature and significance in the development of national security policy. The two volumes under review, by their hasty and

unscholarly preparation, will diminish publishing opportunities for more substantial works in these important areas.

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Woodrow Wilson International  
Center for Scholars

DOUGLAS KINNARD. *The War Managers*. Hanover, N.H.: University Press of New England. 1977. Pp. ix, 216. \$14.00.

Samuel P. Huntington, in reference to Vietnam, once remarked that our policy-makers in future crises would do well "if they [could] simply blot out of their minds any recollection of this one." A retired army brigadier general with two tours in Vietnam, the author of *The War Managers* obviously disagrees. According to Douglas Kinnard, presently a professor at the Woodrow Wilson School at Princeton, retrospective views must be sought so that lessons might be learned.

His study concerns the army general officers who commanded in Vietnam and their opinions as to how the war was managed. He sent a sixty-part multiple-choice questionnaire to the 173 surviving army generals who served in Vietnam during 1965-72. They responded at a sixty-four percent rate, and many added pungent notes in the margins. Mailed in 1974, these confidential questionnaires were completed before the ultimate fiasco in the spring of 1975.

Kinnard's inquiry covered a wide spectrum: national objectives, strategy and tactics, rules of engagement, measurement tools, value of ARVN troops, relations with the media, Vietnamization, and similar topics. To the surprise of Kinnard, the generals, despite their homogeneous backgrounds, showed strong differences of opinion. On only two responses did they express near unanimity: their negative views both of the Army of Vietnam and the media coverage of the war.

Other areas demonstrated divergent views, as shown by the fact that thirty-four percent felt that junior officer leadership improved throughout the conflict, while thirty-two percent thought that this leadership deteriorated as the war progressed. The evaluation of body count statistics demonstrated similar differences, with twenty-six percent feeling that the numbers were reasonably accurate, while sixty-one percent believed that they were often inflated. One general, in response to that question, candidly noted, "The immensity of the false reporting is a blot on the honor of the Army" (p. 75).

Throughout the book, Kinnard provides concise historical commentary on the subjects treated, while writing with a minimum of jargon and passion. His own experiences sometimes surface,

much to the benefit of the book. Discussing the problems of careerism, a subject on which the generals also showed near consensus, Kinnard pointedly charges that many leaders "made a career out of their own careers rather than a career out of leading their own units" (p. 112).

Some readers may query the structure and phrasing of Kinnard's questionnaire. Others may regret that he took time only to hint at some of the concerns and lessons his data produce. If fifty-one percent of the generals doubted the value of search and destroy tactics, if sixty-one percent believed body counts were inflated, if over eighty percent felt careerism was a problem, and if fifty-three percent saw the war as not worth the effort—if so, then where was this level of dissent during the war? Why the silence from so many generals? Or could this negative response find no outlet in an Army that demanded a "can do" spirit? Hopefully in a future volume the author will consider in detail such concerns and lessons. Meanwhile, *The War Managers* provides data and insights for all of us to ponder.

CALVIN L. CHRISTMAN  
Cedar Valley College

ANNE HODGES MORGAN. *Robert S. Kerr: The Senate Years*. Norman: University of Oklahoma Press. 1977. Pp. xiv, 337. \$12.50.

Born in a windowless log cabin in Indian Territory in 1896, Robert Samuel Kerr early in life set out to make a million dollars and become governor of Oklahoma. Achieving the first goal in the 1930s and the second in the 1940s, Kerr was then elected to the United States Senate in 1948; he served there until a fatal heart attack on New Year's Day of 1963. This volume, mostly about Kerr's Senate years, is a worthwhile addition to the growing shelf of American political biographies.

Essentially a man without a political philosophy, Kerr was in the New Deal tradition regarding social welfare legislation, but at the same time he fought on behalf of the oil and gas interests. Entirely eschewing world affairs and directing hardly more than passing notice to the national domestic scene, Senator Kerr unabashedly legislated for the benefit of Oklahoma. One of his major accomplishments was the development of the Arkansas River Navigation System, which was a great economic benefit to northeastern Oklahoma, and his advocacy for the oil and gas business was helpful to Oklahoma's economy as well as to the Kerr-McGee Oil Industries. Kerr was a pragmatic "wheeler-dealer" politician in the tradition of Sam Rayburn and Lyndon Johnson, but his total career reveals that he was not as adept at wheeling and



dealing as were some of his colleagues. With an acerbic tongue, Kerr made enemies and alienated potential friends as he moved roughshod toward his objectives. Without fully understanding the realities of American politics, Kerr made an attempt in 1952 to capture the Democratic presidential nomination. After this debacle Kerr returned to his Senate post more than ever determined to gain more than the Sooner State's fair share of the federal largess. He was a wealthy oil-man who used his wealth to gain political power and who used political power to acquire more wealth.

This adequately written and thoroughly researched volume is essentially a descriptive narrative of the Kerr story. Anne H. Morgan's attempt at total objectivity—letting the facts tell the story—leaves the reader crying out to the author to make a judgment concerning Kerr's political manipulations and at least to raise questions about, if not give answers to, the sticky problem of the morality of Kerr's obsession for protecting and advancing the oil and gas interests. Surely Morgan is entitled to pass tentative judgment upon the career of a man of whom she has intimate knowledge.

MONROE BILLINGTON  
New Mexico State University

EARL WARREN. *The Memoirs of Earl Warren*. Garden City, N.Y.: Doubleday and Company. 1977. Pp. xii, 394. \$12.95.

"How meager is our insight into all but a few" once lamented Felix Frankfurter of his predecessors on the Supreme Court. "On the whole we have a pitifully inadequate basis for understanding the psychological and cultural influences which may be the roots of judicial opinion." Paradoxically, a veritable flood of such material has followed the Frankfurter lament, to such a degree that historians are perilously close to surfeit.

Prototypes of the new genre were Alpheus T. Mason's life of Harlon Stone and Woodward Howard's biography of Frank Murphy. A sub-genre has also emerged: the autobiographical reminiscences of the justices themselves. The latest entry in the latter category is Earl Warren's memoirs, hardly a towering contribution to either law or letters and one which will inevitably suffer by comparison with the crackling and sinewy counterpart efforts of Felix Frankfurter and William O. Douglas.

Notwithstanding a flaccid, lumbering prose and untroubled bucolic certitudes, the Warren memoirs are indeed a rich repository of insights into the roots—and fruits—of judicial opinion. One remarkable example of historical coincidence and

continuity is afforded by the *Whitney v. California* decision in 1927 which came out of the same California court in which Warren was public prosecutor, and which involved a criminal syndicalism act framed by the very legislative committee that Warren had served as clerk. The young lawyer's distaste for what he then encountered obviously foretold the response of a Chief Justice of the United States: "Years later I was therefore pleased as one of my last acts as Chief Justice to join in an opinion invalidating acts of this character." There are some other delightful tidbits: the implausible conversation, high over the North Atlantic, between the incumbent Chief Justice and the former president, Eisenhower, who regarded the latter's appointment as the biggest damn fool mistake he had made. Less delightful is Warren's manifestly shame-faced apology for his role in the Japanese-American relocations where the very words "it was wrong to act without positive evidence of disloyalty. . . ." will always stand in stark contrast to the author's sworn testimony that the absence of positive evidence was the strongest element in the case against the relocatees.

To sum up, this is a manifestly honest effort to tell his story by a good and decent man, and an indispensable guide to the roots of opinion of the Supreme Court that will ever bear his name.

GERALD T. DUNNE  
St. Louis University

WILLIAM J. BOPP. "O. W.": *O. W. Wilson and the Search for a Police Profession*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. ix, 158. Cloth \$9.95, paper \$5.95.

William Bopp has produced a useful study of the life of O. W. Wilson, whose career was closely tied to the movement to professionalize American police departments from the 1920s to the 1960s. While a student at Berkeley, Wilson joined that city's police in 1921 to help his father, who had suffered financial reverses. He became a protégé of Chief August Vollmer, who promoted his career thereafter. Wilson was the kind of man Vollmer wanted as policeman and administrator: honest, intelligent, energetic, and firm. On Vollmer's recommendation, Wilson became chief of the Fullerton, California department, an assignment that turned out badly because Wilson wanted the same kind of independence from political interference that Vollmer had and which Fullerton's political leaders were unwilling to grant.

More successful was his tenure as chief of the Wichita, Kansas department from 1928 to 1939. Unbending toward subordinates he considered corrupt or incompetent, Wilson promoted those



who met his standards both within his own department and as administrators in other cities. Eventually he lost his political support at Wichita and accepted Berkeley's offer to become professor of police administration. During these years Wilson wrote his enormously influential textbook, *Police Administration*, served in the army during and immediately after World War II, and conducted surveys of numerous police agencies. In 1950 the police administration program burgeoned into a School of Criminology with Wilson as dean. When Clark Kerr tried to dismantle the School, Wilson mobilized his professional old-boy network and beat back the challenge.

In 1960 Wilson left Berkeley to take on his most challenging assignment, administering the Chicago Police Department. Contrary to the optimists who thought that he would not last six months, Wilson served for seven years. Until 1965 he enjoyed a good relationship with Mayor Richard Daley, who apparently had decided that intervening in the traditionally scandal-ridden police carried more political costs than benefits. During the last two years of Wilson's tenure his relationship with Daley was strained, although Bopp believes, admittedly on the basis of limited evidence, that Wilson left voluntarily.

In Chicago, Wilson put into practice all that he had learned and taught over the years. He improved the department's equipment and communications, ruthlessly pushed aside seniors in whom he had no confidence, and just as rapidly promoted younger officers he trusted. Wilson did much to improve the image and performance of Chicago's police, although he made a terrible mistake in his chosen successor, James Conlisk. Wilson died in California in 1972.

Bopp, a former police officer and now a professor of police administration, tells the story straightforwardly. He received no cooperation from the Chicago police, and his research on Wilson's army service is thin. Aside from a few letters in the Wilson and Vollmer collections, his most important sources were interviews. In his preface Bopp describes the professionalization movement among the police as a search for identity, and he always measures Wilson's conception of professionalism against his own. Wilson was no democrat, either in his dealings with his subordinates or in his desire to isolate the police from any external control. Yet he did bring a considerable number of blacks into the Chicago department and handled civil rights demonstrations with notable restraint.

Wilson was a hard-working executive with a passion for orderly lines of authority, and he was concerned that the men on the streets execute orders by the numbers. He was a reserved, forbidding man, but one warms to him on reading that

he tried to end the long-standing enmity of FBI Director Hoover, who did not want any competition, by saying in the presence of others, "J. Edgar, what do you say we cut the shit?" Hoover's reply, if any, is lost to history.

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CARL N. DEGLER. *Place Over Time: The Continuity of Southern Distinctiveness*. (Walter Lynwood Fleming Lectures in Southern History.) Baton Rouge: Louisiana State University Press. 1977. Pp. xiv, 138. \$8.95.

Themes of continuity and change, the stuff of historical writing in every field, have become an enduring hot topic in Southern history. This new contribution, Carl Degler's Fleming Lectures, as revised and expanded, comes down on the side of continuity. Degler discounts C. Vann Woodward's emphasis on the sharp breaks in the region's history and favors instead W. J. Cash's metaphor: "The South . . . is a tree with many age rings, with its limbs and trunks bent and twisted by all the winds of the years, but with its tap root in the Old South."

Much of this volume's value derives from the author's gift for synthesis. Degler has kept up with the literature, and the footnotes constitute a guide so up-to-date as to include items still unpublished at the time of writing. In large part, however, the synthesis covers familiar ground. The peculiar elements of Southern character, for instance, are found in the familiar categories of climate, agriculture, rurality, poverty, demographic homogeneity, biracial population, Protestant religion, and the penchant for violence, but more than anything else distinctiveness is rooted in slavery.

Degler cites evidence of a self-conscious South from the Colonial and Revolutionary eras, but like most other writers, he sees a turning point in the 1820s, when the Missouri Controversy and other sectional issues led Southern leaders to embrace in self-defense a strict construction of both the Constitution and the Bible. Along the way the book touches upon such subtopics as the profitability of slavery, the paucity of immigration, the antebellum South's lively interest in commerce and manufactures.

While Degler embraces the idea of Southern distinctiveness, he rejects Eugene Genovese's latter-day version of the Old South as a separate civilization, the stronghold of a divergent society and world view. The notion that a ruling planter class exercised "hegemony," that is, imposed planter values on other classes against their self-interest, rests on assumption rather than evidence,

according to Degler. "In the absence of a test through potential disproof, the concept of hegemony becomes a tautology. All ruling classes exercise hegemony over other classes for that is what a ruling class is by definition." The evidence, indeed, discloses few signs of class conflict. "Many more southerners had a direct interest in slavery than twentieth-century Americans had in the ownership of stocks or in the hiring of workers."

Genovese's invocation of George Fitzhugh as spokesman for the Old South strikes Degler as insupportable, since Fitzhugh was more widely ignored than honored. John C. Calhoun, on the other hand, commanded a large following, and rose to high position. Unlike Fitzhugh, who embraced an organic view of society, Calhoun took a logical and rationalistic view of things in keeping with the framework of Louis Hartz's *Liberal Tradition in America*. Paradoxically, because the South was the only slave society infused with Enlightenment doctrines, it became the only one to emphasize a racial defense of slavery. Doctrines of biological inferiority offered a "surer defense in the American context of equality" than time-honored notions of social orders.

Three out of four chapters deal mainly with the Old South, the final chapter mostly with the remainder of the nineteenth century. There Degler invokes a growing literature showing more continuity of class and racial relations than had previously been recognized—further confirmation of his thesis and also a useful guide to the rising flood of figures on the profitability of sharecropping.

Degler's emphasis on the nineteenth century leaves little space in which to confront the changes of recent years. The transformation in race relations, he says, "goes a long way toward making up for the region's most glaring deficiency." Still, distinctiveness persists, perhaps even in a new-found cooperation between white and black. "Limited though the differences between southerners and other Americans may be," Degler asserts, "they are worth celebrating, as well as recognizing."

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MORTON SOSNA. *In Search of the Silent South: Southern Liberals and the Race Issue*. (Contemporary American History Series.) New York: Columbia University Press. 1977. Pp. xvi, 275. \$11.95.

*In Search of the Silent South* is a competently written and well-researched work that generally upholds the high standards readers have come to expect from books in the Contemporary American History Series. The study examines white southern racial liberalism from the publication in 1885 of

George W. Cable's *The Silent South* to the Brown decision of 1954.

"White Southern liberalism during the Jim Crow era," Morton Sosna writes, "was an attempt to define some nonracist basis for Southern life" (p. 198). On the whole, it proved to be a frustrating task. Some southern "liberals," such as Virginus Dabney, were themselves socially conservative and ended up as defenders of segregation. Others, such as Howard Odum, thought in terms of regional economic development and were quite ambivalent in their view toward blacks. A few, like Lillian Smith, were outspoken opponents of Jim Crow. Reflecting both the diversity of liberal thought and the continuing inability of liberals to awaken "the silent South," southern liberal organizations pursued cautious policies. The Commission on Interracial Cooperation focused its efforts on the prevention of lynchings. The Southern Conference for Human Welfare campaigned against the poll tax. Laudable as both these goals were, neither seriously threatened the caste system in the South. Not until 1949, when the newly created Southern Regional Council came out against the Jim Crow system, did a major white southern organization oppose segregation. Despite the limited achievements of southern liberals, Sosna credits them with helping to "create a climate whereby a federally enforced revolution in the south was actually achieved" (p. 207).

In addition to its merits the work also has limitations. While complementing Carl N. Degler's *The Other South*, Sosna's book lacks Degler's encompassing scope and interpretive insights. Sosna is primarily interested in the urban, intellectual liberals of the university, pulpit, and press. The Populists are brushed over lightly. Moderate politicians, some of whom risked a great deal more than the intellectual liberals, are often ignored. Labor organizational efforts in the South during the New Deal and the post-World War II period are dealt with briefly and are viewed from the perspective of liberal elites. There is no systematic attempt to relate southern liberalism to economic, demographic, and social changes in the South.

But these limitations aside, *In Search of the Silent South* is a good book. It deals perceptively, objectively, and at the same time compassionately with an important group of southerners.

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GLEN JEANSONNE. *Leander Perez: Boss of the Delta*. Baton Rouge: Louisiana State University Press. 1977. Pp. xvii, 440. \$25.00.

Biographies of flamboyant regional political figures have often provided inspired and insightful

writing about the South. C. Vann Woodward's *Tom Watson*, Francis Butler Simkins' *Pitchfork Ben Tillman*, and T. Harry Williams' *Huey Long* are the classics of this genre. At their best such studies not only demythologize their subjects but also crack the neat mold—Dixie demagogue straight from Central Casting—to which they had previously been assigned. Where before we saw one-dimensional characters committed to country music, white supremacy, or the three dollar license plate, we begin to see instead individuals who, despite tragic flaws and with results that were not often worthy of their best efforts, tried to deal with some of the central issues of their, and our, time: economic dislocation, undemocratic politics, and the yearning of poor people for respect and dignity.

Unfortunately, Glen Jeansonne's biography of the Louisiana politico Leander Perez fails to accomplish this, but the fault in this case lies less with the biographer than with his subject. Indeed, Jeansonne has attempted to write a dispassionate yet critical study of Perez, which seeks to explain "the Judge" as the product of a distinctive place—isolated Plaquemines parish located below New Orleans on either side of the Mississippi River as it winds its way to the Gulf—a place that in Perez's lifetime underwent dramatic economic and social change. Perez was an intensely private man, and the author, to his considerable credit, has mined a wide array of sources including oral interviews, obscure legal cases, and Perez papers still in the family's possession. The book is almost certain to be definitive.

Its principal problem as a southern political biography, however, is insoluble: it is about Leander Perez. He emerges, to put it mildly, as too wooden a figure to warrant our close attention. It cannot be said of Perez, as Woodward said of Tom Watson, that "his life was a paradox." In fact, after reading this book, one can only conclude that his life was disturbingly, quite often shockingly, simple. Leander Perez single-mindedly devoted it to two pursuits—getting rich and getting even—modified only by his desire to become richer and remain more than even. Despite Jeansonne's painstaking efforts at fair treatment, his probing exploration reveals a man whose actual life only caricatured an outrageous public image. Perez fanatically opposed state, let alone federal, intrusions into his mineral-rich domain, which through dummy leases to oil companies and other shady dealings was the source of his wealth and power; he concocted unbelievably brazen schemes to flaunt all laws or procedures not of his own making; he pursued, purged, and punished political opponents—it mattered not whether they were local levee board officials or governors, congressmen, or presidents—with ruthless intensity. It was,

then, perfectly consistent for Perez in 1963 to threaten to put any civil rights workers who dared enter Plaquemines into Fort St. Philip, a dilapidated, eighteenth-century floating dungeon so isolated in snake-infested and mosquito-ridden tidal swamps that it was unfit for cattle, not to mention human beings.

Leander Perez, as this study makes clear, was a selfish, unscrupulous bully long before the civil rights movement afforded him the opportunity of becoming one of the South's most notorious bigots. He did not miss his chance. Perez's outbursts against the "niggers," "communists," and "jews" at times caused diehard white supremacists to blush, and in 1962 he was excommunicated from the Catholic Church for his efforts against desegregation. Writing a sensitive biography of such a man is difficult, if not impossible, and while Jeansonne should be praised for trying, Perez's lighter qualities, such as his love of family and paternalistic generosity, more resemble what the late Hannah Arendt once described as "the banality of evil" than the human side of a flawed man.

Still, Jeansonne offers some valuable insights. He makes it clear that Perez, despite his outspoken racism, was the antithesis of a demagogue. He was a behind-the-scenes power-broker, distrustful of the electorate, who exercised considerable influence in Louisiana through money and controlled votes without ever having to seek office outside of Plaquemines and neighboring St. Bernard parish. Nor was he an ignorant, uneducated redneck. A graduate of Tulane Law School, Perez was a master of legal technicalities; his jurisprudential wizardry made him rich and frustrated his enemies. Some even thought him among the smartest men they had ever met.

Perez's death parodied his life. After he had succumbed to a heart attack in 1969, the supposedly excommunicated segregationist was buried with full Catholic rites. In characteristic secrecy, "the Judge" had reconciled with the Church. It was his ultimate backroom deal, and, like most others, one in which he got more than he gave. Those who knew Leander Perez well could not have been too surprised.

MORTON SOSNA

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FRANCES FOX PIVEN and RICHARD A. CLOWARD. *Poor People's Movements: Why They Succeed, How They Fail*. New York: Pantheon Books. 1977. Pp. xiv, 381. \$12.95.

The subtitle of this book indicates the functional purposes the authors intend their study of history

to serve, and their findings may be taken as corroboration of the hypotheses that informed their roles as welfare activists during the 1960s. More than "participant observers" in the welfare-rights movement, they were in fact prime movers of social strategies which constitute the subject matter of this volume's concluding (and most telling) section.

Their fundamental assumptions then—set forth in an article in *The Nation* (May 2, 1966)—argued that a movement to mobilize poor families to demand relief for which they were eligible but for which hundreds of thousands had not registered would, in the short run, provide direct financial aid to the poor and, in the long run, provoke such an acute fiscal crisis that government would be forced to initiate a national system of family income maintenance. Their role within the National Welfare Rights Organization was to argue (with but partial success as it turned out) for tactics of mass protest and disruption rather than of organization and lobbying. Implicit in that preference lay the understanding that the poor, lacking power, cannot rely on tactics appropriate for traditional reform movements, that mass movements once organized tend to become bureaucratic and self-restrained, and that leaders of protest movements are coopted with comparative ease. The implications of these perceptions Piven and Cloward elaborated in *Regulating the Poor* (1971), and in a series of articles gathered in book form, *The Politics of Turmoil* (1974). Now in *Poor People's Movements* they provide extensive historical analysis of four relevant movements—of unemployed workers during the Great Depression, of industrial workers in the 1930s, of oppressed blacks for civil rights, and of the poor for welfare rights.

Given the authors' significant contributions to protest and policy during the 1960s, given the need we all confront to justify our theories and our lives (to ourselves as to others), it is refreshing to find so little of the self-serving or apologetic in this work. The tone is of scholars engaged and concerned, but not dogmatic. The intent is to persuade and to inform, not to polemicize. Footnotes provide running commentary on supporting and conflicting views of other activists and critics on the Left. Their account of the welfare-rights movement, which draws from first-hand experiences remembered now at a certain distance, provides observations and critical insights of primary significance to students of the tumultuous recent past. It is not the kind of book that academic historians are given to writing, but it is one the guild could study with benefit, not least the theoretical opening chapter.

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## CANADA

A. ROSS MCCORMACK. *Reformers, Rebels, and Revolutionaries: The Western Canadian Radical Movement, 1899-1919*. Buffalo: University of Toronto Press. 1977. Pp. xi, 228. Cloth \$15.00, paper \$4.95.

In 1896 a reporter for the *Toronto Globe*, attending a national labor convention in Canada, noted that the western Canadian socialists sat on one side of the room and the eastern trade unionists held aloof on the other. There was no fusion, he said, "and there is very little hope of there ever being any." This splendid book explains more fully than previous studies why the reporter was generally correct in his prediction, and why a vigorous radical movement which emerged in western Canada at the turn of the century failed to achieve its promise. A. Ross McCormack agrees with turn-of-the-century observers and most later commentators that there was a "basic difference of outlook" between workers in eastern and western Canada (p. 127). He uses Dubofsky's thesis to explain why, arguing persuasively that western Canadian miners lived in polarized communities approximating classic Marxian conditions and therefore responded eagerly to revolutionary doctrines during a time of rapid transition to industrial capitalism. Other western Canadians, especially itinerant farm workers or lumberjacks, joined the Wobblies. Both these groups reflected essentially American radical influences; but a third group, the laborites, adopted the moderate reform stance of British workers and pushed for gas and water socialism.

McCormack's early chapters, on the rise to prominence of the Socialist Party of Canada in British Columbia (revolutionaries), the formation of labor parties by British-born trade unionists in Winnipeg and other western cities (reformers), and the Wobblies' march across the border to Canada (rebels), are all thoroughly grounded in a wide variety of materials, including the important and hitherto little-used Mine, Mill Union papers in British Columbia. While clearly illuminating the shifting doctrines and tactics of these radicals, McCormack essentially fleshes out a story whose main outlines already have been drawn in various theses and in the published works of Robin, Phillips, and others.

The author breaks new ground in his analysis of the effects of the First World War on the radicals, and—equally important—of the impact of the radicals' militancy on the Borden government. Inflation, government economic policies, and opposition to national registration united the western Canadian radicals to a point where they "represented the greatest opportunity for significant social change ever to occur in Canada (p. 169). Upon



the defeat of their candidates in the 1917 election at the hands of triumphant Unionists, western workers escalated their militancy and embraced the general strike and industrial unionism. Not just Winnipeg but all western Canadian society was polarized. Workers drew inspiration from the Bolshevik Revolution, and McCormack concludes that the "federal government . . . had ground for fearing that a revolution was beginning in Winnipeg" (p. 166). By showing how the Winnipeg strikers' radicalism reflected a larger pattern of beliefs and experiences endemic to western Canada, and by revealing how their radicalism appeared to the government cloistered in Ottawa, McCormack places Canada's greatest industrial conflict in a new and convincing context and makes the government's repressive measures more understandable if no less repulsive.

Finally, McCormack's book, a well-wrought piece of research which is likely to become essential reading for students of radicalism in North America, greatly reinforces the notion that the CCF sprouted from urban as well as agrarian roots.

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HEATHER GILBERT. *The Life of Lord Mount Stephen*. Volume 2, *The End of the Road, 1891-1921*. Aberdeen: Aberdeen University Press. 1977. Pp. xii, 442. £10.00.

This is the second of Heather Gilbert's two volumes on George Stephen, Lord Mount Stephen (for Vol. I, see *AHR*, 71 [1965-66]: 1474-75). It covers the period between his elevation to the peerage in 1891 and his death, at the age of 92, in 1921. Although he moved to London in 1890 and would not visit North America again after the mid-1890s, he remained an influential figure in the transatlantic system of railroad finance. His greatest interest now was not the Canadian Pacific, from which he largely withdrew, but the Great Northern, being developed with great success out of their old St. Paul, Minneapolis, and Manitoba by his long-time friend, James J. Hill. He maintained his close friendship with Garnet Wolseley; especially important to this volume is his role as godfather to Wolseley's daughter Frances. Following his second marriage, in 1897, to Gian Tufnell, who had been lady-in-waiting to the Duchess of Teck (mother of the future Queen Mary), he developed connections of apparent warmth and significance with members of the royal family.

The most important of his friendships, to this work at least, was that with Gaspard Farrer, a

London banker thirty years his junior; it is largely through Farrer's letterbooks (consulted at Baring Brothers) that Heather Gilbert's story proceeds. Indeed, one of the book's considerable virtues is that it makes available very substantial excerpts from Farrer's correspondence; the effect, however, is to make him its central figure. To an even greater degree than in volume one, the ostensible subject remains a rather shadowy and at best indirectly known figure, whose activities and attitudes are only occasionally, and rather tantalizingly, discussed explicitly. The book follows its sources very closely, often down repetitive or none-too-rewarding paths. Was it, for example, really necessary to say so much about the salmon fishing by guests at Mount Stephen's Quebec lodge, Grand Metis, which he himself almost never used in all the years after 1891?

The book will be of greatest interest to specialists in the history of Anglo-American business, especially railroad finance and politics. Even these, however, will find a good deal of its central story more clearly developed in Albro Martin's recent life of Hill, to which Gilbert had access in manuscript.

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YVES ROBY. *Les Québécois et les Investissements Américains (1918-1929)*. (Cahiers d'histoire de l'Université Laval, number 20.) Québec: Presses de l'Université Laval. 1976. Pp. xii, 250. \$12.50.

At first glance, the title of this book may give the impression that it is a work in the field of economics, but this is not the case. This is a study dealing with the views of Quebecers regarding American investment. To discover these views the author has studied the articles which appeared in the leading French newspapers of the province as well as one English daily, the *Montreal Star*. The years covered by this book, 1918-29, are particularly well suited for the author's purpose, since they saw the entry of a massive flow of American capital into the province of Quebec.

Previous studies of this subject have left the impression that French Canadians resisted industrialization; one gets the idea that the inflow of American capital met with opposition from a society more interested in promoting agriculture and colonization than industrial development. On the contrary, Yves Roby's researches demonstrate that many French-Canadian intellectuals subscribed to a view that identified the progress of the province with industrialization and which welcomed foreign investment. This was certainly the point of view put forward by the Liberal Party



press and by the (Liberal) government of Quebec which set out to attract American funds. By extending his analysis to include the Liberal press, Roby has avoided the trap, into which numerous researchers have fallen, of identifying the thought of French Canada with that of nationalist, conservative, and clerical circles. French Canada as an ideological monolith is a myth; Roby's work confirms this in an even more important way than the title of his book suggests. Behind his analysis of the reaction of Quebecers to American investment one can perceive their attitudes towards industrialization. For the Quebec government, as for the majority of newspapers (in terms of circulation), the future of Quebec was firmly linked to industrial development.

In terms of methodology, the book represents an important contribution. It emerges out of a school of thought developed in Quebec during the past fifteen years which sees economic factors as the fundamental explanation for social developments. Roby has sought to push this mode of analysis a step further by applying it to cultural phenomena. Here economic development becomes the critical factor in determining changes in the perceptions of ideologically diverse factions. In the author's view, the recession which struck Quebec in 1921 and the return to prosperity in 1924 altered newspapers' views of events and underlay changes in attitudes toward industrialization and American investment. But these changes seem to me to be of comparatively minor significance; they had little influence on social thought except to furnish grounds for discussions of some themes rather than others. Could it have been otherwise? Studies of intellectual history indicate that significant changes come very slowly. For any school of thought, it was difficult to perceive important changes in a period of only a dozen years, especially years of social tranquility like the twenties. Nevertheless, Roby's book poses an interesting question: how economic developments can underlie the evolution of ideas. Do we see beyond economic and social history the birth of a new history that will link economics to culture?

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REGINALD H. ROY. *For Most Conspicuous Bravery: A Biography of Major-General George R. Pearkes, V.C., Through Two World Wars*. Vancouver: University of British Columbia Press. 1977. Pp. xiv, 388. \$15.95.

Generations of athletic Englishmen, willing to spend their lives in the service of their country overseas, have been the hallmark of British impe-

rialism. George Pearkes, who emigrated to western Canada in 1906, exemplifies the type. A man of strong character, impressive physique, total fearlessness, and great personal charm, he won a commission in the field on the Western Front in the Great War, and in 1917 earned that most coveted award, the Victoria Cross. He went on to a career in the Canadian Army, entered politics after the Second World War, became Minister of National Defence in John Diefenbaker's ill-starred Conservative government, and eventually accepted the appointment as lieutenant governor of British Columbia.

This was in its later years a rather stormy life culminating with a discreet withdrawal from the center of political affairs. In 1942, having trained the First Canadian Division in England, fully expecting to be given command of I Canadian Corps, Pearkes was instead pushed off to the Pacific Command in Canada. He managed in 1943 to involve a Canadian brigade in the attack on Kiska, an assault that was eventually launched against an enemy who had decamped. He loyally attempted to make volunteers out of the "Zombies" (Home Defence Conscripts) under his command, but was informed in 1944 that he had not made sufficient effort. Perhaps tried beyond patience following a series of highly publicized and embarrassing incidents in late 1944, he resigned his commission in February 1945. As a politician Pearkes enjoyed surprising success, but as a cabinet minister, when he saw that he could never agree with Diefenbaker and his external affairs minister, Howard Green, on nuclear weapon and missile policy, he resigned. One may be permitted to speculate that he found a climate more to his liking as lieutenant governor of British Columbia.

A man who demonstrated such a conspicuous lack of success in areas where he might have made the most impact is a somewhat unusual subject for a biography, especially in view of the scanty documentation available. (There were virtually no private papers, and the book is based largely on taped interviews.) It took courage for a historian who has never before attempted a biography to undertake one on these terms. George Pearkes is too likeable a man to criticize easily. Although Reginald H. Roy has laid out the facts in such a way that the reader can draw his own conclusions, and although Pearkes' superb battle leadership in the First World War comes through very clearly, the author takes his subject very much at face value. Was his skill in the profession of arms and his political acumen really accorded the widespread acclaim that Roy seems to imply? We are, surely, missing the whole man. (One wonders, for instance, what Janowitz would make of such a story.) And yet, there is a great deal to be said for

simply preserving the record of a genuine Henty hero living out his life in the nuclear age. It is quite possible that a more sophisticated account would not have achieved the right effect.

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## LATIN AMERICA

OSCAR LEWIS *et al.* *Four Women, Living the Revolution: An Oral History of Contemporary Cuba*. Urbana: University of Illinois Press. 1977. Pp. xxxviii, 443. \$15.00.

When Cuban prima ballerina Alicia Alonso recently toured the United States, one of the first questions she was asked by the press was, "Is open criticism of the government permitted in Cuba?" To this she replied with apt evasiveness, "No one would want to criticize the regime." This interchange illustrates a persistent tendency on the part of U.S. observers to judge Cuba by liberal bourgeois standards. In *Four Women*, the late Oscar Lewis, his wife Ruth Lewis, and Susan M. Rigdon allow Cubans to speak for themselves. The major theme of the book is: How has the Cuban revolution reshaped the lives of Cuba's women? The introduction addresses the question from a scholarly standpoint; the four autobiographies provide informal yet no less effective testimony. In liberating women the Cuban revolution has made no attempt to create U.S.-style feminists. The liberation of Cuba's women from the traditional constraints of kitchen, Church, and children stems from the fundamental egalitarian thrust of the revolution and is, in itself, a secondary goal. The overriding consideration has been the need to incorporate women into the work force, so that they might actively contribute to building the new Cuba. Ultimately, the aim of the revolution is to liberate women so that they can become "fully integrated" and better serve their community, society, and socialist state.

The extensive interviews on which this book is based were conducted in Cuba in 1969-70 by Lewis and his wife. *Four Women* is one part of a trilogy which proposes to present an "oral history of contemporary Cuba." The autobiographies of four very different women make vivid, dramatic, and compelling reading. Mónica, age twenty-four, middle-class, divorced with two children, university graduate, daughter of a widowed career woman, early political activist; Grácia, age twenty-seven, unmarried, a former nun seduced by a sadistic priest, daughter of a failed, alcoholic poultry dealer, one-time counterrevolutionary; Pi-

lar, age twenty-eight, a mulata, a former prostitute and drug-user, married and divorced twice, mother of two children; Inocencia, age fifty-four, daughter of an impoverished *colono*, one of twelve children, former domestic, divorced. The lives of these women read much like a Zola novel. None—not even the pampered Mónica—had a happy childhood. Family life was a continual torment when poverty disfigured personalities and warped relationships. As children, these women learned their lessons from hunger and deprivation rather than from schoolbooks. And poverty was a savage school. Pilar Lopez got her education at home. At age nine she delivered a seven-month fetus that her mother had purposefully aborted.

Engrossing as these autobiographies are, they do not really provide an "oral history of contemporary Cuba." In fact, we receive only an impressionistic picture of the larger process of revolutionary change. The women's stories add little concrete information about governmental efforts on behalf of women beyond what has been discussed in the introduction. What the autobiographies provide is honest, intimate, and moving testimony to the changes the revolution has wrought in these women's lives. We see the conflicts created—for Grácia and for Inocencia to a lesser extent there was the need to reconcile religion with revolution. We hear complaints and criticism about shortages and hardships. But we also see much of the revolution's real achievement. All the women enjoy a higher level of education than their mothers. The former prostitute, Pilar, was rehabilitated and intended to go on to the university. All attained a new sense of self and feeling of dignity. For Inocencia, a conventional woman in her fifties, the revolution gave her the courage to end an unhappy marriage of twenty-five years and become self-supporting. The greatest legacy of the revolution is hope. These women had known the depths, and, as if reborn, they face the future with optimism and faith. *Four Women* is most effective in conveying this theme.

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TERRY LACEY. *Violence and Politics in Jamaica, 1960-70: Internal Security in a Developing Country*. London: Frank Cass. 1977. Pp. 184. \$22.50.

As the title suggests, this study examines the significance of political violence in Jamaica during the 1960s in the context of all recorded forms of violence—personal, political, and criminal. In attempting to account for Jamaica's relatively high societal instability and violence, Terry Lacey looks at historical as well as contemporary factors that

have contributed to the deepseated economic frustrations of the masses and the persistent material gap (acutely disnomic by the 1960s) between the latter and the national bourgeoisie, who were the principal benefactors of the post-1955 economic development boom.

There is nothing novel about this approach. If anything, Lacey underplays the significance of violence as an essential feature of the slave plantation system and postemancipation Jamaica. But he does make good use of data provided by contemporary socioeconomic studies by Jamaican social scientists to demonstrate the failure of the much-vaunted development program of Bustamante's Labour Party to resolve the island's incapacity to provide enough jobs for its growing and increasingly restless population. The study suggests on the other hand that the opposition Peoples National Party's belated recognition of basic demands of the Black Power/Rastafarian nexus in 1969 averted a possible revolutionary overthrow of the existing political system. Still he leaves little doubt about the final outcome should the present government fail to deal realistically with the basic social and economic frustrations of the black masses.

The study is divided into three sections: 1.) the causes of frustration (largely historical); 2.) manifestations of frustration; and 3.) the forces working to maintain internal security. While some attention is paid to Canada's and the United States' increasing involvement in Jamaica's security after independence, Lacey has little to say about the latter's more covert intervention in the late sixties and early seventies. Nor does the study throw much light on the politicians' involvement in the illegal *ganja* (marijuana) traffic or their exploitation of ghetto violence for their own political ends. There is also some repetition in the narrative (see pp. 42, 123, and 138) and an overuse of such terms as "flashpoint" and "encapsulate."

Nonetheless, Lacey's examination of the close interrelationship between the masses' frustrations and the demonstrated violence of Jamaican society and politics in recent years has helped document a situation sensitive observers have recognized for some time.

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HUBERT J. MILLER. *La iglesia y el estado en tiempo de Justo Rufino Barrios*. Translated by JORGE LUJAN MUÑOZ. (Ediciones Reforma Liberal, number 5.) Guatemala: Universidad de San Carlos de Guatemala. 1976. Pp. 514.

In 1871 the Guatemalan Liberal Party, led by Miguel García Granados and Justo Rufino Barrios,

toppled the Conservative government. García Granados and the more radical Barrios served as the first two presidents (1871-73 and 1873-85) of a Liberal dynasty which lasted until 1944. Barrios in particular comes down to us as one of the major figures of Guatemalan history and his program as one of the outstanding episodes of doctrinaire anticlericalism in nineteenth-century Latin America. It is chiefly these anticlerical measures which Hubert J. Miller documents. Secondly he touches on other aspects of the Liberal reform, particularly the program for secular (and increasingly Positivist) education.

The Liberal anticlerical laws of the 1870s were a repeat of earlier but short-lived efforts under President Mariano Gálvez in the 1830s. Given a second opportunity the Liberals quickly disbanded the religious orders, expropriated most Church property, secularized marriage and education, legalized divorce, proscribed clerical garb in public, reduced or eliminated most sources of Church income, and ended the Church's legal privileges. The hierarchy at first confronted the anticlericals, but when clerical defiance resulted in still harsher measures and the exile of Church leaders, their replacements adopted a policy of accommodation, seeking survival in any form. In 1879 the anticlerical program was written into a new constitution, which remained law until 1944. The Church, deprived of its political and economic power, was left the debilitated shell of its former self.

Miller's study is well founded in original sources, particularly those of the Guatemalan Church. He presents for the first time a detailed and comprehensive account of the Guatemalan Liberal anticlerical program, and considerably enhances our knowledge of a phenomenon of general importance in Latin American history. But in the process he raises more questions than he answers.

For instance, why was anticlericalism so strong in Guatemala? Who (in terms of status, economic interests, etc.) were the leading anticlericals? Miller claims that anticlericalism had few supporters in the general population. But how then was it politically possible to pursue the Liberal program? Conversely, who were the Church's advocates and why could they not rally more support? Was there truth in the Liberal charge that the Church was an economic parasite, misusing theology and clerical authority to milk the people of their few resources? Were Church fees out of line and did these have a connection to anti-Church sentiments among the Indian masses (as the author obliquely suggests on one occasion)?

And what of the Liberal contention that the Church's massive holdings of property blocked economic development? How much property did the Church own? Was it necessary, as the Con-

servatives claimed, to support Church schools and philanthropic institutions? Was the church, which expected control of education, providing an adequate system? Whom did it serve?

These important questions are not answered, and without the answers the anticlerical/clerical struggle loses much of its meaning. Though the evidence may have been incomplete, no one is in a better position than Miller to make an informed, critical attempt to address these questions. Nevertheless, his book is a useful contribution.

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RICHARD MILLETT. *Guardians of the Dynasty: A History of the U.S. Created Guardia Nacional de Nicaragua and the Somoza Family*. Introduction by MIGUEL D'ESCOTO. Maryknoll, N.Y.: Orbis Books. 1977. Pp. 284. \$6.95.

Any study of the Guardia Nacional and the Somoza dynasty in Nicaragua presents special problems. Because of earthquakes, censorship, and inaccessibility of materials, reliable documentary sources are rare. Because of the bitter partisanship of Nicaraguan politics and of the controversy over the involvement of the United States in Nicaraguan affairs few nonpartisan studies of Nicaraguan history exist. Richard Millett's work is the first attempt to analyze objectively the formation of the Nicaraguan Guardia Nacional in the 1920s and Anastasio Somoza's use of it to establish a political dynasty in 1936. Millett makes admirable use of fragmentary information from United States and Nicaraguan archival sources, newspapers, and personal interviews to write this account. He is not entirely successful, however, in reaching his goal of objectivity.

We can best get at the book's limitations by delving directly into the two most important controversies dealt with by Millett. The first is the issue of United States responsibility for Somoza's seizure of power. Millett carefully traces the establishment of the Guardia under United States tutelage. Equally sound is the treatment of the withdrawal of the marines after their failure to eliminate Nicaraguan rebel Augusto César Sandino, the murder of Sandino by the Guardia, and the deepening rivalry between Somoza and President Sacasa. Although Millett treats all of these controversial episodes clearly and objectively, his conclusions are somewhat contradictory. He assigns Somoza much of the blame for the transformation of the Guardia into a partisan instrument of his own personal power but he concludes that "the major share of the responsibility for Somoza's seizure of power . . . must rest with the United

States" (p. 182). This conclusion gives too little weight to the evidence, described in the book, that Washington clearly opposed a partisan Guardia, the murder of Sandino, and even Somoza's accession to the presidency. Short of outright renewal of the military occupation (as unthinkable probably to Millett as it was to President Roosevelt), there was no way to prevent Somoza from taking and consolidating power.

The same dilemma appears in the treatment of the controversy over subsequent diplomatic relations between the United States and Nicaragua. Writers hostile to Somoza frequently blame the United States for upholding the dynasty. In Millett's account, however, it is clear that, at least until the mid-1950s, the United States constantly harassed Somoza rather than upheld him. This harassment involved public criticism, denial of military advice and armament to the extent of influencing third parties to refuse to sell arms to Somoza, and diplomatic pressure to soften his regime. Yet, in the concluding pages Millett refers often to a pattern of Washington coming to the rescue of the dynasty. That United States ambassadors to Nicaragua offered solace and support to the Somoza regime cannot be denied, but this study offers little evidence that the United States invented the Somoza dynasty and sustained it.

CHARLES L. STANSIFER  
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JOHN SAMUEL FITCH. *The Military Coup d'Etat as a Political Process: Ecuador, 1948-1966*. (Johns Hopkins University Studies in Historical and Political Science, Ninety-fifth Series, number 1.) Baltimore: Johns Hopkins University Press. 1977. Pp. xv. 243. \$15.00.

With the publication of his compact analysis of four Ecuadorian military coups in the period 1948 to 1966, political scientist John Samuel Fitch has achieved a significant breakthrough in the study of Latin American militarism. The purpose of Fitch's volume is to use the Ecuadorian case to determine the "decision criteria" for military coups and, more important, to shed light on military motivation in general. Having secured the cooperation of the Ecuadorian military for his study, the author builds his stimulating interpretation on a solid base of interviews along with an extensive bibliography of published works. Fitch's book is a success from start to finish.

This succinct volume is divided into five parts with appropriate chapters. Part one sets forth the major hypothesis of the book and gives an excellent profile of the Ecuadorian armed forces. Part two deals with Ecuadorian history from 1948



to 1966. Having established the historical context, Fitch analyzes the decision criteria for military coups in part three. Part four tests the intensity of each of these criteria along with their interaction and changing importance. Finally, part five summarizes Fitch's findings and deals with their relevance for Ecuadorian history since 1966.

The key concept of the book holds that military men in politics are motivated not by social and geographic origins and affiliations, but by "corporate self-identification" and professional values. (I have set forth the same proposition in my own works on Argentine militarism.) Although the Ecuadorian officer corps is drawn largely from the Sierra middle class, Fitch argues, they have been motivated essentially by corporate military values, a finding which serves a stunning blow to the positions of José Nun and John J. Johnson. Both Nun and Johnson have argued repeatedly that Latin American armies have tended to intervene in politics as surrogates for the civilian middle class or at least their values.

Having stated his core thesis, Fitch goes on to establish the essential decision criteria for Ecuadorian military coups. When the military perceives a "national crisis," he writes, it is influenced by the following judgments: "ratings of the constitutionality of government actions, the officer's personal ties or antagonisms toward the government, public opinion toward the government, government attentiveness to the institutional needs of the armed forces, government policy toward any perceived 'communist threat,' the level of public disorders, and the need for socio-economic reforms" (p. 160). Military perception of these national crises is significantly influenced by four basic "role definitions": professionalist, constitutionalist, arbiter, and developmentalist. With the passage of time, the Ecuadorian military has tended to move from professionalist and constitutionalist to arbiter and socially conscious developmentalist roles.

There are some flaws in this excellent work. For example, Fitch does not treat factionalization and secret lodges in the Ecuadorian military. Nor is there sufficient comparison of the Ecuadorian case with other instances of Latin America militarism. Finally, some consideration of the general "military mind" in Latin America would have been stimulating.

But let us not mince words. Fitch's book is the best case-study yet produced on Latin American militarism; it is a model for future interpretations.

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RAFAEL CALDERA. *Andrés Bello: Philosopher, Poet, Philologist, Educator, Legislator, Statesman*. Translated

by JOHN STREET. London: George Allen and Unwin. 1977. Pp. 165. \$18.75.

Pedro Grases explains in the foreword that the excellence of this book, first published in 1935 by a youth of nineteen (who was to become president of Venezuela), is due to the counsel of one great teacher, Caracciolo Parra León, "although of course we should take into account the qualities and ability of the apprentice author" (p. 8).

Part one of *Andrés Bello*, "The Man," is a sketch of Bello's life. He was born in Caracas, November 29, 1781, only a few blocks from where Bolívar was born July 24, 1783. Bello's family belonged to the petty bourgeoisie. His uncle, a Mercedarian friar, taught him the humanities. Then he entered the university, graduated at the top of his class, studied law for a year, and from 1801-10 served as second officer in the Captain General's Secretariat; but, Caldera writes, "the civil servant never killed the scholar" (p. 21). The 1810 Junta sent Bello to London as secretary of the diplomatic mission which Bolívar headed. They arrived there July 10, so how could Bello have been commissioned on August 10 (p. 23)?

Bello remained in England for nineteen years. He was married twice in London and at times barely earned a living, but he continued to write and study. From 1822-24 he served as secretary to the Chilean legation and then became secretary to the Colombian legation. He wanted to be Colombian minister to the United States but failed to get this appointment, perhaps because his own generation slandered him for remaining in England while they fought for independence.

In 1829 Bello accepted the Chilean Government's invitation to come to Chile. He arrived there on June 25, and for the rest of his life (thirty-four years) "ruled the intellectual and pedagogic life of that nation in a period which is regarded as one of the most brilliant in Spanish America."

Part two, "The Sage," describes six facets of Bello: philosopher, poet, philologist, jurist, educationalist, and sociologist. Each is one aspect of a brilliant whole, the complete scholar. In the chapter "The Sociologist," Caldera relates part of Bello's criticism in 1844 of José Victorino Lastarria's study *Investigaciones sobre la influencia de la conquista y del sistema colonial de los españoles en Chile*. In this study Lastarria presented Chile's colonial history from a critical/philosophical point of view. I wish Caldera had gone one step further with Bello's criticism, as Luis Galdames did, to stress Bello's dictum that historical studies of a philosophical character have importance only when the facts they are based upon have been proved.

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STEPHEN J. RANDALL. *The Diplomacy of Modernization: Colombian-American Relations, 1920-1940*. Buffalo: University of Toronto Press. 1977. Pp. xi, 239. \$15.00.

This book by a Canadian historian examines United States-Colombian relations between the wars in order to test, in this relatively neglected case, some of the established propositions concerning the nature of the Good Neighbor policy. Stephen J. Randall argues that "the fundamental issue (in those years) was whether Colombia would pursue a road to development independent of the United States" (p. 16), and as such it presaged the struggle between the industrialized and the less developed countries following the Second World War.

For Randall, United States diplomacy was successful in the interwar period in keeping Colombia within the ambit of a traditional export economy largely dependent on the United States both for trade and investment. Contrary to the emphasis in some other analyses, however, Randall stresses the continuities both with earlier U.S. policy and between the Hoover and Roosevelt administrations. In his view, the Good Neighbor policy is merely a tactical departure from previous policy, brought on by the perception in the United States that overt intervention was a counterproductive way to achieve its goals in the face of Latin American sensibilities. Coincident with such an argument, Randall maintains that it was not primarily the threat of Hitler that induced changes in U.S. policy, but rather a longer-term recognition of the inefficacy of earlier tactics.

The book is, however, by no means a simplistic denunciation of U.S. imperialism. For, according to the author, the inherent disparities of international power and the internal dynamics of Colombia's own political economy determined the outcome as much as any actions by private or public actors in the United States. Nor is the U.S. government viewed as an unequivocal supporter of U.S. private interests (e.g., United Fruit); rather, the State Department is depicted as having often attempted to press conciliation on U.S. firms for the sake of larger (i.e., security) considerations. For Randall, in fact, it appears to be security (of the Panama Canal and the Caribbean region generally) that took precedence in U.S. policy whenever such concerns tended to conflict with the claims of U.S. investors.

The book is organized topically, stressing some often-neglected economic and financial issues as well as the somewhat better-known problems relating to United Fruit, petroleum concessions, and the German-controlled Colombian airline. If the book at times degenerates into long recitals of the

details of fairly routine diplomatic negotiations, overall it is well written, scholarly, thorough, and judicious. It also contains two bonuses: it manages to convey some real sense of the conflict among Colombian interest groups themselves over foreign economic policy, and it has a very useful bibliographical essay. In all, Randall's book is a welcome contribution to the literature.

ROBERT H. DIX  
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JEFFREY L. KLAIBER. *Religion and Revolution in Peru, 1824-1976*. (International Studies of the Committee on International Relations.) Notre Dame, Ind.: University of Notre Dame Press. 1977. Pp. xii, 259. \$16.95.

For decades historians have analyzed Peru's rich and complex historical tradition from a myriad of interpretive viewpoints, but Jeffrey L. Klaiber's study offers a totally new focus, one which is both provocative and controversial. Stressing that the only thing held in common by all Peruvians is their acceptance of the tenets of the Catholic Church, Klaiber sets out "to examine the interaction between the religious world views of the reformist elite and the Peruvian lower classes" from 1824 to 1976.

Klaiber concludes that spokesmen for reform, most of whom were anticlerical, ended up adopting lower-class religious symbols while "lower class Peruvians reinterpreted their traditional religiosity by identifying it with the cause of social justice and by infusing their religious symbols with a new, revolutionary sense" (p. 5).

Implicit in this provocative thesis, however, is the controversial assumption that the surest and most successful route for achieving social justice is through the Catholic Church and, concomitantly, that the only true reform in the past was based to some degree on Christian humanism.

Klaiber begins with an overview of nineteenth-century anticlericalism and then treats Manuel González Prada as the intellectual link between that century and the social movements of the twentieth. In subsequent chapters, however, Klaiber tries too hard to fit his data to his thesis.

In discussing nineteenth-century Indian uprisings, Klaiber praises the role and importance of local priests in helping the Indians and concludes that "the Indians had come to experience Catholic Christianity and social protest as compatible realities" (p. 69). Likewise, the author overemphasizes the religious component in the *indigenista* movement and then offers the highly debatable conclusion that José Carlos Mariátegui was either "a Christian in search of a new faith. Marxism, or . . .

a convinced Marxist returning to the Christianity of his youth" (p. 92).

One encounters this same problem in Klaiber's discussion of Víctor Raúl Haya de la Torre and the APRA Party. I do not question the importance of popular Christianity as practiced by the Aprista masses, nor do I question the fact that Haya sought to take political advantage of these beliefs by adopting Christian-type symbols and rhetoric. I do have serious reservations, however, about Haya's sincerity and about the real Christian content of Aprismo.

The final chapter on the Church and the military, 1945-76, is far too ambitious in scope and thus lacks the attention to detail and unity of thought which characterize the rest of the work.

In sum, this is an interesting book which offers a unique interpretation of Peruvian history. Though certainly controversial in places, Klaiber's central thesis on popular religiosity must at least be considered by future historians.

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BRIAN LOVEMAN. *Struggle in the Countryside: Politics and Rural Labor in Chile, 1919-1973*. (International Development Research Center, Studies in Development, number 10.) Bloomington: Indiana University Press. 1976. Pp. xxxvi, 439. \$12.50.

Brian Loveman's massive study exhibits solid conceptualization, rigorous original research, clear writing, and usually persuasive analyses. For Chileanists, it offers the most thorough and authoritative coverage of the agrarian question in the twentieth century. Over half the book examines in depth the three decades preceding the 1960s. Particularly impressive is his analysis of the evolution of the legal aspects of property and labor rights. For non-Chileanists, valuable insights abound on agrarian political issues of concern throughout Latin America and the Third World. For example, the treatment of rural/urban linkages, both within the upper class and among manifold other political actors, is intricate and instructive.

Indeed, Loveman argues cogently that Chile's exceptionally democratic political tradition resulted in large part from a tacit bargain between the landed elites and urban reformers. That understanding maintained a precarious representative system of government in an underdeveloped economy of scarcity by excluding the rural poor from effective participation. When that enforced subordination of agricultural labor broke down in the sixties and seventies, so did the existing property relationships and political accommodations. To make this important argument about changes over time, it is necessary to establish, but risky to

exaggerate, the scope and impact of past peasant activism and repression.

Loveman asserts that "only when organized rural labor participates actively in the political struggle in the countryside is there the possibility of a comprehensive redefinition of property and an effective redistribution of property rights" (p. 20). This belief leads to his conclusion that "if the campesinos are to be successful they must trust no government, no party, no coalition, and no caudillo" (p. 333). In line with these contentions, the author rejects both Marxian statist agrarian reforms and any notion of a resigned, tradition-bound, or relatively passive peasantry. Instead, he uncovers repeated instances of peasant protests and resistance, if not rebellion, stretching back to the opening decades of the twentieth century. With a marvelous grasp of history, this political scientist builds his significant revisionist case out of the previously unmined riches in the national labor archive, interviews with peasant leaders, and a vast bibliography. There is no doubt left that discontent has been felt and expressed by agricultural workers more than most writers have assumed or implied.

Unfortunately, Loveman at times weakens his position by overselling the militancy and influence of rural workers, especially during the 1930s and 1940s. For example, he claims that labor mobilization and strikes in the countryside were "massive" and came in "waves," but his own findings indicate they were small and came in spurts. Furthermore, he neglects electoral data. As Loveman notes, rural laborers throughout most of the twentieth century constituted the electoral foundation of the more conservative parties. He correctly but perhaps excessively blames this voting behavior on the multiple means of coercion and bribery employed by the landowners. The persistent reluctance of rural workers to cast ballots for candidates promising radical agrarian reform also raises serious questions about how many peasants could or did actively attack or defy the hacienda class. Predictably in agrarian societies, the political activities, consciousness, and leverage of most tillers of the soil probably remained fairly localized. As Loveman himself shows with admirable skill and objectivity, it was above all changes generated from the growing urban center that eventually made peasant grievances part of a national drive for structural transformations in property and power relationships. Thanks to his penetrating study, a great deal more is now known about the dynamic interaction between that national political process and its rural victims and beneficiaries.

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JOSÉ RIBEIRO JÚNIOR. *Colonização e monopólio no nordeste Brasileiro: A Companhia Geral de Pernambuco e Paraíba (1759-1780)*. (Coleção Estudos Brasileiros, number 3.) São Paulo: HUCITEC. 1976. Pp. 210.

Brazil played a critical role in sustaining Portugal's economic well-being during the eighteenth century. Brazilian sugar, tobacco, cacao, hides, dyes, and rare timber provided profitable re-exports for the mother country, and Brazilian gold paid for the importation of manufactured goods. In 1779 one Portuguese government official went so far as to claim that "Portugal without Brazil is an insignificant power."

Yet the Portuguese were aware that the entrepôt function could be of much greater value to them if commercial transactions, credit, and navigation, all of which remained largely in the hands of foreigners, could be nationalized. During the 1750s, seeking to escape from their dependence on economically more powerful European neighbors, and looking to the example of English and French mercantilists, the Portuguese established a number of monopolistic commercial companies. The economic history of one of these enterprises, the Company of Pernambuco and Paraíba, which between 1759 and 1780 was granted exclusive rights to the commerce and navigation of the Brazilian northeast is the subject of this excellent monograph by José Ribeiro Júnior, professor of the history of Brazil at the Faculty of Philosophy, Science, and Letters of Assis in São Paulo.

Based on previously unexploited records of the company in the archive of the Ministry of Finance in Lisbon, supplemented by research in the Overseas Historical Archive in Lisbon and elsewhere, this work is wholly original and a fine example of the high level of contemporary Brazilian historical

scholarship. Ribeiro's study provides detailed insights into the commercial mechanisms and techniques, business methods, and policy objectives of the eighteenth-century Luso-Brazilian world. There is much new material here for specialists: data on the profitability, mortality rates, and numbers involved in the Angola-Pernambuco slave trade; a revision of previous views of the mid-century Luso-Brazilian economic recession which the Pernambuco data suggests has been exaggerated; a delineation of the close links between the company and industrial development in Portugal, and the company's role in stimulating the growth of a national mercantile and industrial bourgeoisie in Portugal. Ribeiro also points up what he sees as the company's major shortcoming: its over-extension of credit. He finds that within seven years indebtedness to the company in the northeast had already surpassed the total value of four years' production of the region. The collection of these debts after the abolition of the company's monopoly was, he suspects, an element in the series of republican uprisings in Pernambuco during the early nineteenth century. In fact, as late as 1907 he finds the "extinct company" pursuing its debtors in the northeast.

The author, however, never loses sight of the broader context of his study. The Marquês de Pombal, who dominated the Portuguese government between 1750 and 1777, played a critical role in the establishment and operation of the Pernambuco company. Ribeiro's book is a fundamental addition to the growing body of new material which will soon make essential a major reassessment of the history of eighteenth-century Portugal and Brazil in general, and the politics of Pombal in particular.

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## Festschriften and Miscellanies

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

DAVID H. BAYLEY, editor. *Police and Society*. (Sage Focus Editions, number 1.) Beverly Hills, Calif.: Sage Publications. 1977. Pp. 260. Cloth \$15.00, paper \$7.50.

DAVID R. JOHNSON, A Sinful Business: The Origins of Gambling Syndicates in the United States, 1840-1887. ROBERT D. STORCH, Police Control of Street Prostitution in Victorian London: A Study in the Contexts of Police Action. PHILIP JOHN STEAD, The New Police. DEBORAH L. JAKUBS, Police Violence in Times of Political Tension: The Case of Brazil, 1968-1971. LAWRENCE W. SHERMAN, Police Corruption Control: Environmental Context versus Organizational Policy. WILBUR R. MILLER, Never on Sunday: Moralistic Reformers and the Police in London and New York City, 1830-1870. JAMES LEO WALSH, Career Styles and Police Behavior. JOHN P. CLARK, RICHARD H. HALL, and BRUCE W. HUTCHINSON, Interorganizational Relationships and Network Properties as Contextual Variables in the Study of Police Performance. WILLIAM A. GAMSON and EPHRAIM YUCHTMAN, Police and Society in Israel. DAVID H. BAYLEY, The Limits of Police Reform. RICHARD E. SYKES, A Regulatory Theory of Policing—A Preliminary Statement.

ALBERT F. ELDRIDGE, editor. *Legislatures in Plural Societies: The Search for Cohesion in National Development*. (Publications of the Consortium for Comparative Legislative Studies.) Durham, N.C.: Duke University Press. 1977. Pp. xv. 284. \$12.75.

MALCOLM E. JEWELL, Legislative Representation and National Integration. RICHARD SISSON and LAWRENCE L. SHRADER, Social Representation and Political Integration in an Indian State: The Legislative Dimensions. MARVIN G. WEINBAUM, The Legislator As Intermediary: Integration of the Center and Periphery in Afghanistan. LENARD J. COHEN, Conflict Management and Political Institutionalization in Socialist Yugoslavia: A Case Study of the Parliamentary System. IAN BUDGE and CORNELIUS O'LEARY, Permanent Supremacy and Per-

petual Opposition: The Parliament in Northern Ireland, 1921-72. ALLAN KORNBERG and SAMUEL M. HINES, Parliament's Role in the Integration-modernization of Canadian Society, 1865-76. ABDO I. BAAKLINI, Legislatures and Political Integration in Lebanon: 1840-1972.

ITHIEL DE SOLA POOL, editor. *The Social Impact of the Telephone*. (MIT Bicentennial Studies, number 1.) Cambridge, Mass.: MIT Press. 1977. Pp. viii, 502, \$15.95.

SIDNEY H. ARONSON, Bell's Electrical Toy: What's the Use? The Sociology of Early Telephone Usage. ASA BRIGGS, The Pleasure Telephone: A Chapter in the Prehistory of the Media. CHARLES R. PERRY, The British Experience 1876-1912: The Impact of the Telephone During the Years of Delay. JACQUES ATTALI and YVES STOURDZE, The Birth of the Telephone and Economic Crisis: The Slow Death of the Monologue in French Society. COLIN CHERRY, The Telephone System: Creator of Mobility and Social Change. ITHIEL DE SOLA POOL, CRAIG DECKER, STEPHEN DIZARD, KAY ISRAEL, PAMELA RUBIN, and BARRY WEINSTEIN, Foresight and Hindsight: The Case of the Telephone. JOHN R. PIERCE, The Telephone and Society in the Past 100 Years. HENRY M. BOETTINGER, Our Sixth-and-a-Half Sense. JOHN BROOKS, The First and Only Century of Telephone Literature. MARTIN MAYER, The Telephone and the Uses of Time. ALAN H. WURTZEL and COLIN TURNER, Latent Functions of the Telephone: What Missing the Extension Means. BRENDA MADDOX, Women and the Switchboard. SUZANNE KELLER, The Telephone in New (and Old) Communities. JEAN GOTTMAN, Megalopolis and Antipolis: The Telephone and the Structure of the City. RONALD ABLER, The Telephone and the Evolution of the American Metropolitan System. J. ALAN MOYER, Urban Growth and the Development of the Telephone: Some Relationships at the Turn of the Century. BERTIL THORNGREN, Silent Actors: Communication Networks for Development. A. J. L. REID, Comparing Telephone with Face-to-Face Contact. EMANUEL A. SCHEGLOFF, Identification and Recognition in Interactional Openings. DAVID LESTER, The Use of the Telephone in Counseling and Crisis Intervention. PALADUGU V. RAO, Telephone and Instructional Communications.

FILIPPO RANIERI, editor. *Rechtsgeschichte und quantitative Geschichte: Arbeitsberichte*. (IUS Commune.

Veröffentlichungen des Max-Planck-Instituts für Europäische Rechtsgeschichte. Sonderhefte, nummer 7.) Frankfurt am Main: Vittorio Klostermann. 1977. Pp. xi, 153. DM 38.50.

F. RANIERI, Versuch einer quantitativen Strukturanalyse des deutschen Rechtslebens im 16.-18. Jahrhundert anhand einer statistischen Untersuchung der Judikatur des Reichskammergerichts. Ein Arbeitsplan. P. C. TIMBAL, L'exploitation des archives du Parlement de Paris: une méthode et ses résultats. G. DOLEZALEK, Computer und Rechtsgeschichte. Einführung und Literaturübersicht. DELLOYD J. GUTH, How Legal History Survives Constitutional History's Demise: the Anglo-American Tradition.

B. MITCHELL SIMPSON III, editor. *War, Strategy, and Maritime Power*. New Brunswick, N.J.: Rutgers University Press. 1977. Pp. x, 356. \$19.50.

BERNARD M. W. KNOX, Herodotus, Thucydides, and the Problem of Power. GORDON B. TURNER, The Nature of War. SIR BASIL H. LIDDELL HART, The Objective in War. NORMAN H. GIBBS, Clausewitz on the Moral Forces in War. HERBERT ROSINSKI, New Thoughts on Strategy. HENRY E. ECCLES, The Basic Elements of Strategy. JAMES A. FIELD, JR., The Origins of Maritime Strategy and the Development of Seapower. WILLIAM REITZEL, Mahan on Use of the Sea. JOHN HATTENDORF, Technology and Strategy. NORMAN RICH, National Interest in Imperial German Foreign Policy: Bismarck, William II, and the Road to World War I. MARTIN BLUMENSON, Links between World Wars I and II. STEPHEN E. AMBROSE, Seapower in World Wars I and II. B. MITCHELL SIMPSON III, The Rearming of Germany 1950-1954: A Linchpin in the Political Evolution of Europe. HENRY E. ECCLES, Suez 1956: Some Military Lessons. JOHN LEWIS GADDIS, The Cold War: Some Lessons for Policymakers. ROBERT L. PFALTZGRAFF, JR., The Evolution of American Nuclear Thought. ALLAN E. GOODMAN, The Future of United States Development Assistance in the Insurgency Environment. WILLIAM A. HAMILTON III, The Decline and Fall of the Joint Chiefs of Staff. JOHN M. LEE, United States Military Roles in a Period of Resource Scarcity.

DUMBARTON OAKS PAPERS, number 30. Washington: Dumbarton Oaks Center for Byzantine Studies; distributed by J. J. Augustin, Locust Valley, N.Y. 1976. Pp. xiv, 400.

HÉLÈNE TOUBERT, "Rome et le Mont-Cassin": Nouvelles remarques sur les fresques de l'église inférieure de Saint-Clément de Rome. FRANCIS NEWTON, The Desiderian Scriptorium at Monte Cassino: The *Chronicle* and Some Surviving Manuscripts. HENRY M. WILLARD, The Staurotheca of Romanus at Monte Cassino. KURT WEITZMANN, The Ode Pictures of the Aristocratic Psalter Recension. DAVID PINGREE, The Byzantine Version of the *Toledan Tables*: The Work of George Lapithes? DAVID PINGREE, Political Horoscopes from the Reign of Zeno. NICOLAS OIKONOMIDES, Leo VI and the Narthex Mosaic of Saint Sophia. NICOLAS OIKONOMIDES, Leo VI's Legislation of 907 Forbidding Fourth Marriages: An Interpolation in the *Procheiros Nomos* (IV, 25-27). ANDRZEJ POPPE, The Political

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HEER H. LIEBAERS, Voorafgaande beschouwingen door. M.-A. ARNOULD, Considérations préliminaires. P. AMELUNG, Der Ulmer Buchdruck im 15. Jahrhundert. Quellenlage und Forschungsstand. L. DESGRAVES, L'introduction de l'imprimerie dans le Sud-Ouest de la France jusqu'à la fin du XVI<sup>e</sup> siècle. S. CORSTEN, Ulrich Zell als Geschäftsmann. L. BALSAMO, Imprese tipografiche in Emilia nel sec. XV: aspetti economici. J. PIROZYNSKI, Cracow the Center of Polish Fifteenth and Sixteenth Century in Printing. P. COCKSHAW, Les textes monétaires imprimés sous le règne de Philippe le Beau (1482-1506). H. ROLOFF, Der Gesamtkatalog der Wiegendrucke. I. KOTVAN, Wiegendrucke, die in Bratislava gedruckt wurden. A. SKOVAN, L'imprimeur Bozidar Vukovic et son temps (fin XV<sup>e</sup> siècle-1539). F. M. S. GUEDES, Lignes de force de l'imprimerie portugaise au XVI<sup>e</sup> siècle. G. BORSA, Druckorte und Papiermühlen des 15. und 16. Jahrhunderts in Ungarn. O. VALLS I SUBIRA, L'influence de l'imprimerie sur la fabrication du papier en Catalogne pendant les XV<sup>e</sup> et XVI<sup>e</sup> siècles. M.-A. ARNOULD, Quand sont apparus les premiers moulins à papier dans les anciens Pays-Bas. L. HELLINGA, Problems about technique and methods in a fifteenth century printing house (Nicolaus Ketelaer and Gherardus de Leempt, Utrecht, 1473-1475), appendix by W. HELLINGA. J. VEYRIN-FORRER, Les premiers ateliers typographiques parisiens. Quelques aspects techniques. L. GILISSEN, Contribution à l'archéologie du livre incunable.

G. E. AYLMER and REGINALD CANT, editors. *A History of York Minster*. New York: Oxford University Press. 1977. Pp. xv, 586. \$22.50.

ROSALIND M. T. HILL and CHRISTOPHER N. L. BROOKE, From 627 until the Early Thirteenth Century. BARRIE DOBSON, The Later Middle Ages, 1215-1500. ERIC A. GEE, Architectural History until 1290. JOHN H. HARVEY, Architectural History from 1291 to 1558. Appendix: The Architects of York Minster from 1290 to 1558. CLAIRE CROSS, From the Reformation to the Restoration. DOROTHY M. OWEN, From the Restoration until 1822. OWEN CHADWICK, From 1822 until 1916. DAVID E. O'CONNOR and JEREMY HASELOCK, The Stained and Painted Glass. PETER ASTON, Music since the Reformation. G. E. AYLMER, Funicular Monuments and other Post-Medieval Sculpture. C. B. L. BARR, The Minster Library. REGINALD CANT, From 1916 until 1975.

WILLIAM P. BUNDY, editor. *Two Hundred Years of American Foreign Policy*. (A Council on Foreign Relations Book.) New York: New York University Press. 1977. Pp. xi, 251. \$15.00.

FELIX GILBERT, Bicentennial Reflections. ALASTAIR BUCHAN, Mothers and Daughters (Or Greeks and Romans). GORDON A. CRAIG, The United States and the

European Balance. JOHN PATON DAVIES, America and East Asia. GEORGE F. KENNAN, The United States and the Soviet Union, 1917-1976. ABRAHAM F. LOWENTHAL, The United States and Latin America: Ending the Hegemonic Presumption. CHARLES P. KINDLEBERGER, U.S. Foreign Economic Policy, 1776-1976.

RICHARD L. EHRLICH, editor. *Immigrants in Industrial America, 1850-1920*. Charlottesville: University Press of Virginia, for Eleutherian Mills-Hagley Foundation and the Balch Institute. 1977. Pp. xiv, 218. \$12.50.

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# Other Books Received

BOOKS LISTED WERE RECEIVED BY THE *AHR* between February 1, 1978 and March 30, 1978. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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- BOTEIN STEPHEN, *et al.*, editors. *Experiments in History Teaching*. Cambridge, Mass.: Harvard-Danforth Center for Teaching and Learning. 1977. Pp. 117. \$3.50.
- BUCHANAN, GEORGE. *The Politics of Culture*. London: Menard Press; distributed by Serendipity Books, Berkeley, Calif. 1977. Pp. 54. £1.80.
- CARROLL, JOHN. *Puritan, Paranoid, Remissive: A Sociology of Modern Culture*. Boston: Routledge and Kegan Paul. 1977. Pp. xi, 148. \$8.95.
- CLARK, GRAHAME. *World Prehistory in New Perspective*. 3d ed., illustrated. New York: Cambridge University Press. 1977. Pp. xx, 554. Cloth \$29.95, paper \$9.95.
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- ERICKSON, CHARLOTTE, editor. *Emigration from Europe, 1815-1914*. (Documents in Economic History.) Atlantic Highlands, N.J.: Humanities Press. 1977. Pp. 320. \$13.00.
- FARB, PETER. *Humankind*. Boston: Houghton Mifflin. 1978. Pp. xiii, 528. \$15.95.
- FREEMAN, DONALD M., editor. *Foundation of Political Science: Research, Methods, and Scope*. New York: Free Press. 1978. Pp. xiv, 882. \$29.95.
- HULTON, PAUL, *et al.* *The Work of Jacques Le Moyne de Morgues: A Huguenot Artist in France, Florida, and England*. Volume 1, *Foreword, Catalogue and Introductory Studies*; volume 2, *The Plates*. London: British Museum Publications in association with the Huguenot Society of London, for the Trustees of the British Museum. 1977. Pp. xii, 241; ix, 144 plates. £75.00.
- KING, PRESTON, editor. *The Study of Politics: A Collection of Inaugural Lectures*. Totowa, N.J.: Frank Cass. 1978. Pp. xiv, 322. \$27.50.
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- TAYLOR, JOHN. *Arator, Being a Series of Agricultural Essays, Practical and Political: In Sixty-Four Numbers*. Edited by M. E. BRADFORD. Indianapolis: Liberty Fund. 1977. Pp. 394. Cloth \$9.00, paper \$3.00.
- WILLIAMS, L. PEARCE, and STEFFENS, HENRY JOHN, editors. *The History of Science in Western Civilization*. Volume 1, *Antiquity and Middle Ages*; volume 2, *The Scientific Revolution*; volume 3, *Modern Science, 1700 to 1900*. Washington, D.C.: University Press of America. 1977. Pp. 338; 348; 500. \$9.75; \$9.75; \$11.50.

## ANCIENT

- JONES, TOM B. *From the Tigris to the Tiber: An Introduction to Ancient History*. Rev. ed. Homewood, Ill.: Dorsey Press. 1978. Pp. xiv, 377. \$7.95.

## MEDIEVAL

- RIBBE, WOLFGANG, and SCHUTZE, JOHANNES, editors. *Das Landbuch des Klosters Zinna*. (Studien zur europäischen Geschichte. Zisterzienser-Studien, number 2.) Berlin: Colloquium Verlag. 1976. Pp. 216.
- RUSSELL, FREDERICK H. *The Just War in the Middle Ages*. (Cambridge Studies in Medieval Life and Thought. Third Series, number 8.) New York: Cambridge University Press. 1977. Pp. xi, 332. Paper \$9.95.
- VAN DEN NIEUWENHUIZEN, J. *Oorkondenboek van het Sint-Elizabethshospitaal te Antwerpen (1226-1355)*. (Académie Royale de Belgique. Commission Royale d'Histoire.) Brussels: Palais des Académies. 1976. Pp. xxxviii, 414.

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- COPE, ESTHER S., editor, in collaboration with WILLSON H. COATES. *Proceedings of the Short Parliament of 1640*. (Camden Fourth Series, number 19.) London: Royal Historical Society. 1977. Pp. 339. £5.50.
- HORROCKS, BRIAN, with EVERSLEY BELFIELD and H. ESSAME. *Corps Commander*. New York: Charles Scribner's Sons. 1978. Pp. xi, 256. \$14.95.
- HUME, JOHN R. *The Industrial Archaeology of Scotland*. Volume 2, *The Highlands and Islands*. Toronto: Macmillan of Canada. 1977. Pp. 335. \$14.95.
- MCHARDY, A. K. *The Church in London, 1375-1392*. (London Record Society Publications, number 13 for the year 1977.) London: The Society. 1977. Pp. xxiv, 126.
- MANLEY, FRANK, editor. *St. Thomas More: A Dialogue of Comfort against Tribulation*. (The Yale Edition of the Works of St. Thomas More. Selected Works.) New Haven: Yale University Press. 1977. Pp. 1,331. Cloth \$22.50, paper \$6.95.
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- WATNEY, JOHN. *The Churchills: Portrait of a Great Family*. London: Gordon and Cremonesi; distributed by Atheneum Publishers, New York. 1977. Pp. 168. \$17.95.
- WATT, D. E. R. *A Biographical Dictionary of Scottish Graduates to A.D. 1410*. New York: Oxford University Press. 1977. Pp. xlii. 607. \$66.00.
- WILSON, DEREK. *The World Encompassed: Francis Drake and His Great Voyage*. New York: Harper and Row. 1977. Pp. xiii, 240. \$12.95.

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- BOCHNER, JAY. *Blaise Cendrars: Discovery and Re-creation*. (University of Toronto Romance Series, number 32.) Buffalo: University of Toronto Press. 1978. Pp. 311. \$22.50.
- COORNAERT, EMILE. *Destins de Clio en France depuis 1800: Essai*. Paris: Les Editions Ouvrières. 1977. Pp. 190.
- CROMÉ, FRANÇOIS. *Dialogue d'entre le maheustre et le manant*. Edited by PETER M. ASCOLI. (Les Classiques de la Pensée Politique, number 10.) Geneva: Librairie Droz. 1977. Pp. 232.
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- ELEK, HÉLÈNE. *La mémoire d'Hélène*. (Actes et mémoires du peuple.) Paris: François Maspero. 1977. Pp. 311.
- PETITFILS, JEAN-CHRISTIAN. *Le gaullisme*. (Que Sais-Je?) Paris: Presses Universitaires de France. 1977. Pp. 126.
- REX, WALTER E. *Pascal's Provincial Letters: An Introduction*. New York: Holmes and Meier. 1977. Pp. 84. \$4.50.

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- Documentos del Archivo de Campomanes: Discurso crítico-político sobre el estado de literatura de España y medios de mejorar las universidades y estudios del Reyno*. Edited by JOSÉ E. GARCÍA MELERO. (Publicaciones de la Fundación Universitaria

- Española. Documentos históricos, number 1.) Madrid: Fundación Universitaria Española. 1974. Pp. 47.
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- KEEFE, EUGENE K., et al. *Area Handbook for Portugal*. (Foreign Area Studies, American University.) Washington, D.C.: U.S. Government Printing Office. 1977. Pp. xiv, 456.
- LLAMS, ENRIQUE, editor. *Documentación inquisitorial: Manuscritos españoles del siglo XVI existentes en el Museo Británico*. (Publicaciones de la Fundación Universitaria Española. Documentos históricos, number 3.) Madrid: Fundación Universitaria Española. 1975. Pp. 143.

## GERMANY, AUSTRIA, AND SWITZERLAND

- DA POZZO, FRANÇOIS. *Die Schweiz in der Sicht des Auslandes: Ein Forschungsbericht über die politikwissenschaftliche Literatur des Auslandes zum politischen System der Schweiz*. (Helvetica Politica, Series B, number 11.) Bern: Francke Verlag. 1977. Pp. 256. 36.00 FR.
- FABER, HAROLD, editor. *Luftwaffe: A History*. New York: New York Times Books. 1977. Pp. xvii, 267. \$15.00.

## ITALY

- BETTONI, MARGHERITA, editor. *Le relazioni diplomatiche fra lo Stato pontificio e la Francia*. Third Series, 1848-1860. Volume 3. 16 aprile 1850-10 novembre 1853. (Fonti per la Storia d'Italia, Documenti Diplomatici.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1976. Pp. xiii, 437. L. 10,000.
- KEEFE, EUGENE K., et al. *Area Handbook for Italy*. (Foreign Area Studies, American University.) Washington, D.C.: U.S. Government Printing Office. 1977. Pp. xiv, 296.
- QUELLER, DONALD, with FRANCIS R. SWIETEK. *Two Studies on Venetian Government*. Geneva: Librairie Droz. 1977. Pp. 175.
- STELLA, ALDO, editor. *Nunziature di Venezia*. Volume 2, 26 maggio 1571-4 luglio 1573. (Fonti per la Storia d'Italia, Nunziature d'Italia. Secoli XVI-XVIII.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1977. Pp. xxxvi, 509. L. 10,000.
- VALIANI, LEO. *Il Partito socialista italiano nel periodo della neutralità, 1914-1915*. Rev. ed. (Universale Economica.) Milan: Feltrinelli Economica. 1977. Pp. 176. L. 2,000.

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- LYONS, MARVIN. *Russia in Original Photographs, 1860-1920*. Edited by ANDREW WHEATCROFT. New York: Charles Scribner's Sons. 1978. Pp. xii, 212. \$20.00.
- SERCZYK, WŁADYSŁAW A. *Iwan IV Groźny* [Ivan the Terrible]. Wrocław: Ossolineum. 1977. Pp. 176. 40 Zł.

## NEAR EAST

- NYROP, RICHARD F., et al. *Area Handbook for the Persian Gulf States*. Washington, D.C.: U.S. Government Printing Office. 1977. Pp. xiv, 448.
- NYROP, RICHARD F., et al. *Area Handbook for Saudi Arabia*. 3

- ed. Washington, D.C.: U.S. Government Printing Office. Pp. xiv, 389. \$6.30.  
 PHILBY, H. ST. J. B. *Arabia of the Wahhabis*. Reprint. Totowa, N.J.: Frank Cass. 1977. Pp. xiv, 422. \$25.00.

## AFRICA

- BLAKE, JOHN W. *West Africa: Quest for God and Gold, 1454-1578*. 2d ed. (Previously published as *European Beginnings in West Africa, 1454-1578*.) Totowa, N.J.: Rowman and Littlefield. 1977. Pp. xxi, 246. \$9.00.  
 FORBATH, PETER. *The River Congo: The Discovery, Exploration and Exploitation of the World's Most Dramatic River*. New York: Harper and Row. 1977. Pp. xii, 404. \$15.00.  
 KONCZACKI, Z. A., and KONCZACKI, J. M. editors. *An Economic History of Tropical Africa*. Volume 2, *The Colonial Period*. Totowa, N.J.: Frank Cass. 1977. Pp. xv, 260. \$24.00.  
 TABLER, EDWARD C. *Pioneers of Natal and Southeastern Africa, 1552-1878*. (South African Biographical and Historical Studies.) Cape Town: A. A. Balkema; distributed by ISBS, Forest Grove, Ore. 1976. Pp. 117. \$14.50.

## ASIA

- CHING, JULIA. *Confucianism and Christianity: A Comparative Study*. New York: Kodansha International, for Institute of Oriental Religions, Sophia University, Tokyo. 1978. Pp. xxvi, 234. \$12.50.  
 KLING, BLAIR B. *Partner in Empire: Dwarkanath Tagore and the Age of Enterprise in Eastern India*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xii, 276. \$12.50.

## UNITED STATES

- ADAMS, WILLI PAUL. *Die Vereinigten Staaten von Amerika*. (Fischer Weltgeschichte, number 30.) Frankfurt am Main: Fischer Taschenbuch Verlag. 1977. Pp. 532.  
 ADLER, JAMES B., editor. *Comprehensive Index to the American State Papers and Serial Set Publications of the 15th to 34th Congresses (1789-1857)*. In three volumes. (CIS US Serial Set Index, part 1.) Washington, D.C.: Congressional Information Service. 1977. Pp. xx, 942; viii, 1141; vii, 583. \$400.00 the set.  
 ASHTON, JEAN W. *Harriet Beecher Stowe: A Reference Guide*. (Reference Guides in Literature.) Boston: G. K. Hall. 1977. Pp. xxii, 168. \$18.00.  
 BAUER, K. JACK, editor. *Soldiering: The Civil War Diary of Rice C. Bull, 123rd New York Volunteer Infantry*. San Rafael, Calif.: Presidio Press. 1977. Pp. x, 259. \$12.95.  
 BRADLEY, FRANCIS. *The American Proposition: A New Type of Man*. New York: Moral Re-Armament. 1977. Pp. ix, 175. \$6.50.  
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## Recent Deaths

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OSCAR EDWARD ANDERSON, JR., a life member of the Association, died after a heart attack in his home in Bethesda, Maryland on April 14, 1976. At the time of his death he was director of the international program policy division of the National Aeronautics and Space Administration in Washington, D.C.

Anderson was born in South Bend, Indiana on July 3, 1918. He attended public schools in South Bend and Oberlin College, where he graduated summa cum laude in history in 1940. After receiving a master's degree in history at Harvard in 1941, he joined the U.S. Army Air Corps and served as an intelligence officer with the Sixth Bomber Command in the Panama Canal Zone.

After World War II, with encouragement from Frederick Merk, Anderson returned to Harvard to pursue a Ph.D. in history. Studying under Merk and Arthur M. Schlesinger, Sr., Anderson followed the main line of American political history, but only to establish a firm base for pursuing his own special interest: the impact of technology on American history. His doctoral dissertation, published as *Refrigeration in America* in 1953, was the first of three works on this theme. Reread today, Anderson's first book is clearly a pioneering work in the history of technology. In that book he showed a mastery of technology far beyond that of a general historian of that day and also an ability to work in a much broader scope than most specialists attempted. Firmly rejecting "hardware history," Anderson insisted that the history of technology made sense only in terms of its contributions to an understanding of larger themes. This conviction led him always to view the historian's craft as directly applicable to all areas of human endeavor, however technical.

After receiving his Ph.D. at Harvard in 1948, Anderson began his teaching career at the University of Cincinnati, where he continued his study of the impact of technology. While his first book focused on social and economic effects, his new interest carried him into the relationships between politics and technology. His biography of Harvey W. Wiley, published in 1958, traced the career of

the Indiana physician and chemist to its zenith in the Pure Food and Drugs Act of 1906. Anderson presented not only a biographical study but also lucid expositions of the growth of higher education in the sciences in the Midwest, the maturing of chemistry as a profession, the growing importance of science in the federal government, and its role in the Progressive movement. The last of these themes he analyzed in an article in the *American Historical Review* in April 1956.

Before Anderson completed the Wiley biography he was already deeply immersed in a study of a new technology, one that then seemed likely to transform American society. Early in 1958 he was invited to participate in writing a history of the U.S. Atomic Energy Commission. For a tenured professor, the risks of accepting such an offer were high. His courage in embracing that opportunity brought him into a position to write about and to participate in some of the grandest events in the history of the twentieth century. The conventional career which he forswore could only have rewarded him in more ephemeral ways. Anderson accepted the challenge and threw himself wholeheartedly into the new project. He had a major role not only in establishing the approach, format, and style of the historical series but also in structuring the Commission's historical program. In scarcely more than three years Anderson and his coauthor completed the manuscript of a comprehensive history of the development of atomic energy in the United States during World War II. Published in 1962 as *The New World, 1939-1946*, this first volume in the AEC historical series was neither the administrative history of a government agency nor a specialized study in nuclear science and technology. Rather it followed the approach of Anderson's earlier works in showing the many ways in which a new technology could change the patterns of American life. Anderson's part in creating this book was a tour de force, remarkable both for the enormous amount of energy and the extraordinary intellectual discipline which he brought to the project.

Scarcely had Anderson completed the first vol-

ume in the AEC series than he decided to accept a still more demanding assignment, one which would carry him farther from the traditional role of the historian. In 1962 he joined the international program policy division in NASA and became director of the division two years later. In this position he played a key role in developing space cooperation with the Soviet Union, an effort which culminated in the successful Apollo-Soyuz joint docking mission in July 1975. For his contributions, Anderson received NASA's Exceptional Service Medal. He had earlier received outstanding performance awards from the AEC and NASA.

Anderson was a gifted historian, a dedicated public servant, and an indefatigable pursuer of the truth. He demanded of himself the very highest standards in accomplishment, excellence, and integrity. This consuming passion for the best molded his life. His contributions to our understanding of the role of science and technology in American history will long live after him.

RICHARD G. HEWLETT, *AEC*, and  
A. HUNTER DUPREE, *Brown University*

The field of modern French history lost one of its most well-known and well-liked members on June 5, 1977, when RICHARD M. BRACE collapsed and died on a tennis court at Chauvenet (Yvelines), France. The author of numerous works on revolutionary France, the Maghreb in the colonial period, and French Caribbean colonies, Richard Brace was doing research in the Paris archives on West Indian slavery before a heart attack overcame him at age sixty-one. His many friends will recognize the spirit of the man in a letter written several days before his death, which, recounting the trials of arriving in Paris during a general strike, concludes: "But who cares? This is my city and always will be."

Until his first trip to France at age twenty-four Richard Brace had lived all his life in the San Francisco Bay area, attending the University of California at Berkeley for undergraduate and graduate studies. There he worked under Franklin C. Palm, Herbert E. Bolton, and James Westfall Thompson. With his new wife, Joan, he arrived in France for doctoral dissertation research in the first week of September 1939, spent the Sitzkrieg period working under difficult conditions in the archives of Bordeaux, and took ship in the last days of the Third Republic the following June. The result of this stay was *Bordeaux and the Gironde, 1789-1794*, first published in 1947 and recently reprinted. A landmark monograph, this was one of the first works by an American scholar to emulate Georges Lefebvre's studies of the revolutionary pe-

riod outside Paris. The book pioneered a type of regional investigation that has since become one of the leading approaches in work on the French Revolution.

After receiving the Ph.D. degree from Berkeley, Richard Brace first taught for two years each at the University of Maine and Pomona College, with time out as a Naval Agent in a Japanese-language school. After the war he moved to the University of Colorado and then to Northwestern University, where he was to stay for eighteen years. In 1965 he came, as chairman of the history department, to Oakland University, then a new, experimental branch of Michigan State University, and remained on the Oakland faculty until his death. He taught also as a visiting professor at the University of California, San Diego, in 1968-69.

In addition to producing seven articles on various aspects of the French Revolution, published before 1957, Brace also developed a strong interest in the history of French North Africa. As the Algerian crisis consumed the Fourth Republic, he began to search for connections between 1789 and twentieth-century revolutionary movements. Eventually three books were the fruits of this concern: *Ordeal in Algeria* (1960), *Algerian Voices* (1965), both of which were co-authored with Joan Brace, and *Morocco, Algeria, Tunisia* (1964). The earliest of these books was probably the first widely read work in America to treat the Algerian revolutionary movement to a fair hearing. Supported by Fulbright, Social Science Research Council, and Rockefeller Foundation fellowships, the Braces traveled widely in Europe and the Maghreb, met with many of the emerging North African leaders, established the Algerian Children's Fund to help war orphans, and wrote with sympathy and scholarship of the region's history and politics. In what proved to be the last stage of his career, Brace was inspired by the recent literature on the history of slavery to undertake a comparative study of the law and practice of slavery in France and the French West Indies from the seventeenth to the nineteenth centuries. The short pieces he and his wife have produced on this subject suggest the outlines of a highly original full-scale work which Joan Brace intends to bring to completion.

As a teacher, one of Richard Brace's most influential contributions was a Western Civilization textbook, *The Making of the Modern World*, which was widely used in survey courses during the late fifties and the sixties. Through it many thousands of students were touched by Brace's learning, but especially fortunate were those who encountered him in person. A man of wonderful wit, charm, and enthusiasm, his impact on students was the sum of his knowledge and extraordinary character. The doctoral students he trained at Northwestern

well remember his warm concern for their professional careers and personal lives. So too do his younger colleagues at Oakland, many of whom he recruited while chairman. Indeed, Richard Brace never seemed happier than when he could do something to advance the career of a junior scholar. The strong interest he took in two recently organized groups, the Western Society for French History and especially the French Colonial History Society, which he helped found, reflect his concern with the creation of outlets for new historians to communicate the results of their research. He was also an active member of the Société des Études Robespierriennes and the American Historical Association, and he served as vice-president of the Society for French Historical Studies in 1968–69. In these and other groups his voice will be greatly missed and long remembered, no less than in a faculty whose members were proud to call him colleague.

JOSEPH KLAITS  
Oakland University

L. ETHAN ELLIS, Voorhees Professor of History emeritus at Rutgers University and an outstanding scholar in the field of American foreign relations died on October 14, 1977, at the age of seventy-nine. He was born in Otisco, New York and graduated Phi Beta Kappa from Syracuse University in 1920. After teaching for three years at Emory University, he entered the Graduate School of the University of Chicago, where he received his doctorate in 1927. He then taught for one year at Purdue University before joining the Rutgers faculty. He served as chairman of the department of history from 1951 to 1957, after which he became graduate adviser. He was a visiting professor at the University of Rochester, 1953–54, and following his retirement from Rutgers in 1963 he held similar posts at the University of Illinois, Georgetown University, and Seton Hall University.

A disciplined and tireless scholar whose major publications included: *A History of the Chicago Delegation in Congress* (1931), *Reciprocity 1911* (1940), *Print Paper Pendulum* (1948), *A Short History of American Diplomacy* (1951), *Newsprint* (1960), *Frank B. Kellogg and American Foreign Relations* (1961), *Republican Foreign Policy, 1921–1933* (1968), and *Forty Million Schoolbooks Can't Be Wrong* (1975). Ellis was a careful craftsman with a lucid prose style; his numerous contributions to the study of twentieth-century American foreign policy command lasting respect for their solid substance and authoritative judgments.

Modest in his demeanor and diffident about his own impressive scholarly accomplishments, Ethan Ellis held a unique place among his students, col-

leagues, and friends because of his extraordinary kindness and admirable integrity. Because of the general respect for his fair-mindedness and dedication, he was called on constantly to fill positions of responsibility within the university and the community. Despite the arduous demands that he made on himself, he always had time to listen to the concerns of his students and colleagues and to lend his friendly counsel and assistance. He exemplified the best in our profession.

RICHARD P. MCCORMICK  
Rutgers University,  
New Brunswick

EDWARD THADEUS FLOOD, associate professor of history at the University of Santa Clara, died of cancer in Santa Clara, December 11, 1977, at the age of forty-five. He is survived by his widow, Chadin, and a daughter, Theresa.

Born March 15, 1932, Thad Flood served as a Marine infantryman in the Korean conflict. He earned a Bachelor of Philosophy degree from Seattle University in 1958 and then entered upon the study of Asian history at the University of Washington, where he met and married Chadin, at the time a Thai graduate student. He received Fulbright-Hayes fellowships to study in Japan, 1963–65, and Thailand, 1965–66. He earned his doctorate in 1967, with his dissertation "Japan's relations with Thailand: 1921–1941."

In September 1966, Thad Flood joined the Department of History at Santa Clara, where he taught East and Southeast Asian History.

His careful scholarship early bore fruit. He and his wife edited the *Dynastic Chronicles, Bangkok Era, The Fourth Reign*, with commentary, in five volumes, published by the Center for East Asian Culture, UNESCO, Tokyo, 1965–68. They continued working on this series, and the first volume of the *Dynastic Chronicles, Bangkok Era, First Reign* is due for publication in March 1978. Chadin Flood means to continue their work.

In addition he published several articles, including "Sukhothai-Mongol Relations: a Note on Relevant Chinese and Thai Sources," *Journal of the Siam Society* (July 1969); "The 1940 Franco-Thai Border Dispute and Phibun Songkhroam's Commitment to Japan," *Journal of Southeast Asian History* (September 1969); and "The Shishi Interlude in Old Siam," in David Wurfel, ed., *Meiji Japan's Centennial: Political Thought and Action* (1971).

Respected by his colleagues at Santa Clara, Thad Flood received in 1970 early promotion to associate professor, and served as chairman of the department of history (1971–73).

What more and more preoccupied Thad Flood was a deep concern for the relations of the United

States and its people with the peoples of East and Southeast Asia. While preparing a course on Indo-China in 1967, he came to question United States policy there, and soon became active in the anti-war movement. In 1970 students and faculty members at Santa Clara, reacting against the United States "incursion" into Cambodia, sent Thad Flood as a member of their delegation to Washington, D.C. to protest the incursion. There his doubts about the attitudes and qualifications of those determining United States Asian policy seemed confirmed.

He became convinced that deep-seated cultural and political attitudes obscured for most Westerners the realities of Asia. Field trips in 1974 to remote Thai peasant communities intensified this belief.

As he developed these convictions, he submitted them to the most rigorous standards of criticism and scholarship, which were reinforced by his superb linguistic ability. In addition to the standard Western languages, he read and spoke Japanese and Thai, read Chinese, and when he died was learning to speak Pekinese. He came to call himself a "Marxist," although he considered dogmatic Soviet Marxism another form of Western imperialism. Some Asian Marxists seemed to him to be developing a more human socialism, but he was not certain. He joined the committee of Concerned Asian Scholars and wrote for its *Bulletin*.

In consequence of the military coup d'état of October 1976 in Thailand, Flood found himself increasingly involved with Amnesty International, investigating allegations of violations of human rights by the new Thai regime. Here as always, every statement Thad Flood made rested upon thorough and painstaking research.

He embarked upon the yet larger task of cross-cultural comparative history, in hopes of enlightening his countrymen about themselves as well as about the peoples of East and Southeast Asia. Beneficiaries were not only his students, who packed his classes so long as his strength permitted him to give them, but also his faculty colleagues. He used the give and take of faculty seminars to develop and test his stimulating ideas. I am certain he was about to become a world historian of the most exciting kind, and thus I believe our loss is truly great.

In the spring of 1976 Thad Flood learned that he had incurable cancer. He bravely faced his imminent death and persevered in his work as he could, buoying his spirits by sailing, his favorite pastime, when able. His work only ceased three weeks before his death, when he entered the hospital for the last time. Thad Flood's courage, moral and scholarly integrity, and commitment to mankind remain to inspire his family, his col-

leagues and student, the Thai community, and all others who knew him.

PETER PIERSON  
*University of Santa Clara*

DAVID HARRIS was born in Texas on November 27, 1900. He received his baccalaureate (1924) and A.M. (1925) degrees from Stanford University. En route to his Stanford Ph.D. degree in history (1928) he studied and researched in Paris, Berlin, and Vienna. He taught for two years (1928-30) at the University of South Carolina and then in 1930 joined the Stanford History faculty. Except for a visiting professorship at Cornell University in 1940-41 and for membership on the staff of the Department of State during 1942-47, he continued at Stanford until 1966 when he became professor emeritus. He died on August 18, 1975 in Palo Alto, California following a brief illness.

Harris specialized in modern European history in his teaching and scholarly work. His earliest research and first publication, *Diplomatic History of the Balkan Crisis of 1875-1878* (1936), dealt with the backstage maneuverings during the initial phases of the 1875-78 crisis in European affairs. Of this volume, a reviewer observed that Harris "has produced a more skilfully organized, better written, and more detailed account of the first year of the crisis than has yet appeared, so far as the reviewer knows, in any language." His second book, *Britain and the Bulgarian Horrors of 1876* (1939), described the Bulgarian insurrection of 1876, its cruel suppression by Turkish power, and the attitudes these events generated in the British government, press, and population. The reviewer for the *AHR* commended Harris for his ability "effectively, sometimes ironically, to expose the shams which politicians and even journalists perpetrated" in dealing with the problems created by the Turkish government in its effort to arrest the disintegration of the Ottoman Empire in the Balkan peninsula.

*The Bulgarian Horrors* appeared on the eve of World War II, and for Harris, as for others, the war interrupted his scholarly pursuits. He planned to return to Stanford at war's end in 1945; but the State Department urgently requested that his leave be extended, as it was, because of his involvement with important and sensitive work related to defining United States policy toward the political resettlement of postwar Europe. He shared much of the burden of designing postwar national boundaries while vigorously and courageously opposing proposals for dismembering Germany. His views did not prevail; nevertheless, when Germany was partitioned, as political adviser to principal officials, he loyally spent two years endeavoring to preserve and advance both



American and German interests in the divided nation. Given the course of subsequent events, the question might well be asked how postwar developments would have differed had Harris' views become policy.

After the war, busy as always with students and solicitous of their welfare, and busy, too, with service on university committees, Harris still found time to write articles and book reviews for professional journals. He directed his major attention, however, to the preparation of a general history of Europe since 1750. For those who knew his intellectual range and grasp, his undertaking of such an enterprise would come as no surprise; unhappily for him and the profession, he was unable to complete it. An outcome of the study for which the scholarly world owes him a debt was his brilliant essay, "European Liberalism in the Nineteenth Century," published in the *AHR* (1955). This piece preeminently reflected his deep faith in the humane, rational approach to society's problems.

It is not enough to sketch the record of assiduous and distinguished scholarship; of generous service to his profession and to teaching, to his alma mater, and to his country; and his solicitous concern for the welfare and advancement of his students. It remains to say something of David Harris himself. His seriousness as a scholar and teacher could not conceal his sense of whimsy or his ready capacity to evoke and enjoy laughter. He possessed the gift of a light touch which was evident in his conversation, in his professional writing, and in his personal correspondence. He was a man of quiet, genuine courage, not one to push for personal advantage, but when confronted with what he regarded as an injustice to someone else, he fought to correct that injustice. His scholarship, his courage, his selflessness, his devotion to students, his gift of wit and whimsy—all these we remember. But of all our treasured memories of David Harris, none will abide more vividly than the warmth and constancy of his friendship.

GEORGE HARMON KNOLES  
*Stanford University*

EDGAR EUGENE ROBINSON, Margaret Byrne Professor of American History emeritus at Stanford University, died at his Palo Alto, California home September 7, 1977. Born in Oconomowoc, Wisconsin April 5, 1887, he earned his A.B. (1908) and his A.M. (1910) degrees at the University of Wisconsin. He taught at Carleton College during 1910-11 and in 1911 accepted an assistant professorship at Stanford University, where he devoted the remainder of his life to scholarship, teaching, and administration.

For the first dozen years Robinson taught pri-

marily early American history, the westward movement, and the history of the Far West—as befitted a former student of Frederick Jackson Turner. More and more, however, his interests turned to the study of American political parties and to presidential leadership. After the publication, with Victor J. West, of *The Foreign Policy of Woodrow Wilson, 1913-1917* (1917), Robinson brought out *The Evolution of American Political Parties; A Sketch of Party Development* (1924). His *The Roosevelt Leadership, 1933-1945* (1955) grew out of a J. Brooks B. Parker award made to produce a contemporary appraisal of Roosevelt's presidency. Believing that leaders should propose carefully reasoned programs and then enlist support for them among the electorate, Robinson deplored F.D.R.'s experimental approach to the solution of American economic and political problems. The book was reprinted in 1972 following the reissue in 1970 of two of his related publications, *The Presidential Vote, 1896-1932* (originally published in 1940) and *They Voted for Roosevelt* (first published in 1947). Robinson's long-time interest in Herbert Hoover eventually flowered in *Herbert Hoover, President of the United States*, with Vaughn Davis Bornet (1975). Hoover epitomized Robinson's conception of leadership, and the work elaborated this conception and implied that Hoover's failure to win re-election in 1932 represented the country's repudiation of a method of presidential leadership best suited to a democratic republic.

Robinson's scholarly concerns were accompanied by a life-long commitment to liberal education. When during the 1910s and 1920s Stanford reviewed its free elective system, he played a leading role in the drastic restructuring of the university's undergraduate program; henceforth, students in their first two years were expected to devote considerable time to liberal studies. Then in 1923, Stanford required all lower division students to take an interdisciplinary course, "Problems of Citizenship," which Robinson organized and directed for the dozen years of its existence. In 1935, the university substituted the "History of Western Civilization," offered within the history department, in place of "Problems of Citizenship" as the required course. Meanwhile, Robinson had become executive head of the history department, and in that post he exercised close oversight of the new course and of its staff of instructors. His contribution to the Independent Study Plan, established at Stanford in 1925, was equally remarkable; he became chairman of the supervising committee in 1928, a position he occupied for sixteen years. The innovation was designed to provide special opportunities for students of exceptional ability, at the same time freeing them from many of the normal routines of academia. During the life of the

plan—a precursor of honors programs so popular in the 1960s and '70s—hundreds of Stanford students became involved with individually tailored programs of study and writing guided by departmental advisers.

Robinson enjoyed rare gifts as a speaker, and from the beginning of his career he was in great demand on the lecture platform. In the classroom he freely used these gifts in ingenious ways to arouse student interest. In the seminar room as well as in the lecture hall, he stimulated and provoked inquiry and discussion among his students. He attracted a large number of graduate students who earned their doctorates under his direction; many of them helped perpetuate the Turnerian influence so evident in Robinson's own teaching.

There is much more that might be mentioned about Robinson at Stanford: his tenure on the Board of Directors of the Hoover Library on War and Peace; the organization of the Institute of American History under his direction and its sponsorship of summer conferences and seminars for secondary school teachers of American history; leadership which he gave to the History Department as executive head for twenty-three years; the honor of holding one of the first (1931) endowed professorships at Stanford and as the honored person of the third such professorship—the Edgar E. Robinson professorship in United States History (1945); and the substantial number of contributions he made to his academic discipline. Pub-

lications not hitherto mentioned include *American Democracy in Time of Crisis* (1934); *Independent Study at Stanford* (1937); *The New United States* (1946); *Scholarship and Cataclysm* (1947); as coeditor with Paul Carroll Edwards, *The Memoirs of Ray Lyman Wilbur, 1875-1940* (1960); and as editor and contributor, *Powers of the President in Foreign Affairs* (1966). Robinson was also a visitor on the faculties of Yale, Columbia, and the universities of Minnesota, Michigan, Oregon, and California. He was a fellow of the Royal Historical Society of Great Britain and a member of the American Historical Association—he was president of the latter's Pacific Coast Branch in 1928. His alma mater conferred an honorary degree of Doctor of Laws upon him in 1942.

Robinson, who served under the first five Stanford presidencies and continued his professional activities during two more, was a mainstay of the institution for over forty years. The full impact of his many and varied contributions to the development of Stanford and through it to education generally defy exact calculation. He was, as a newspaper editor recently put it, "an old style academic giant." We therefore honor the memory of Edgar Eugene Robinson—pioneer Stanford professor, vigorous scholar, inspiring teacher, educational statesman, cherished friend.

GEORGE HARMON KNOLES  
*Stanford University*



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# American Historical Association

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**MEMBERSHIP:** Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 14,500. Members elect the officers by ballot.

**MEETINGS:** The Association's annual meeting takes place on December 28-30. The meeting in 1978 will be held in San Francisco, California. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, the bibliographic series *Writings on American History*, and *Recently Published Articles*. To promote history and assist historians, the Association offers other services, including an Institutional Services Program and the quarterly publication of the *Employment Information Bulletin*. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

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**CORRESPONDENCE:** Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

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# American Historical Review

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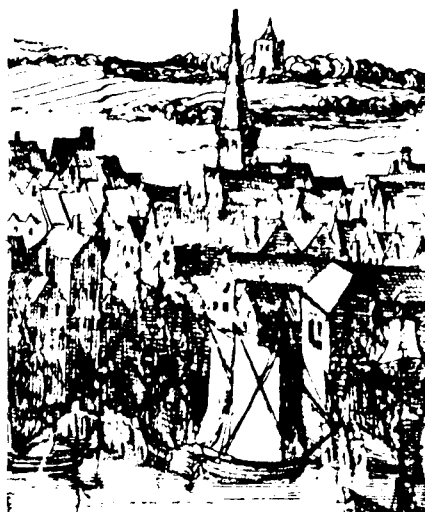
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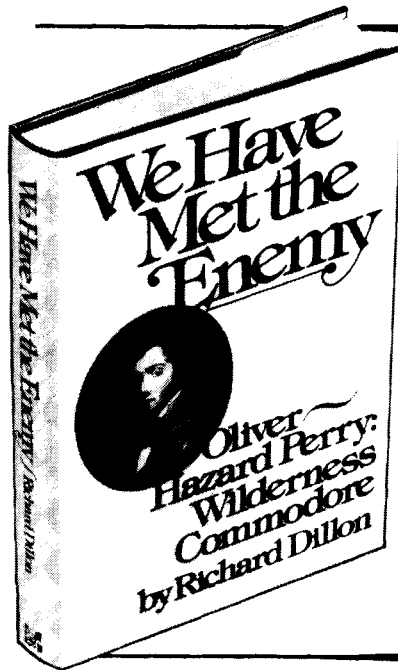
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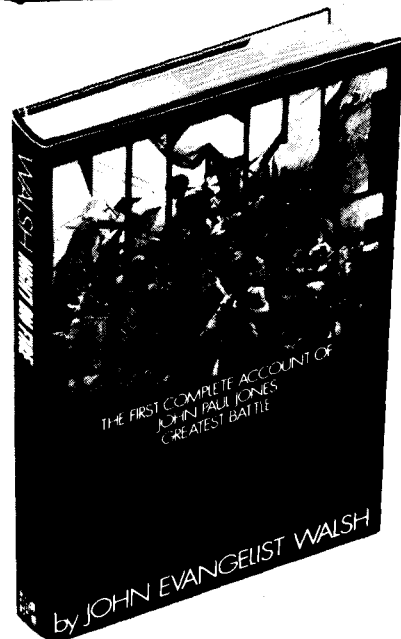
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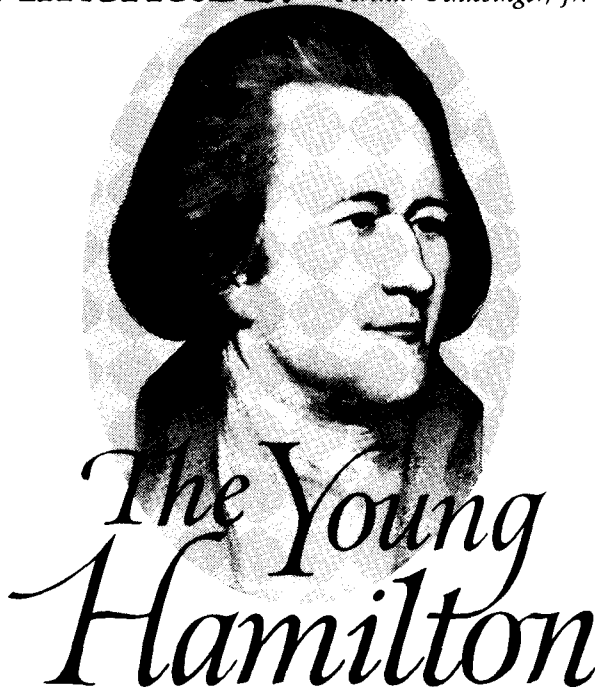
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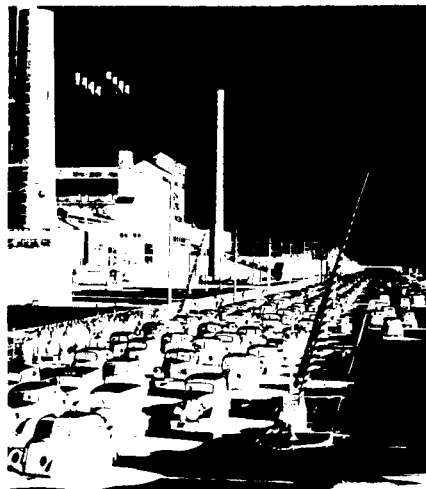
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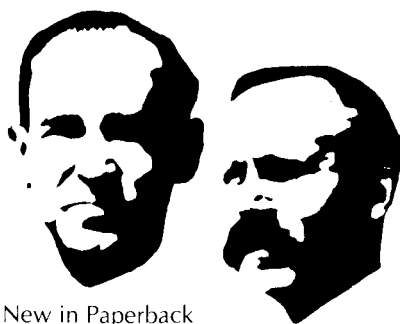
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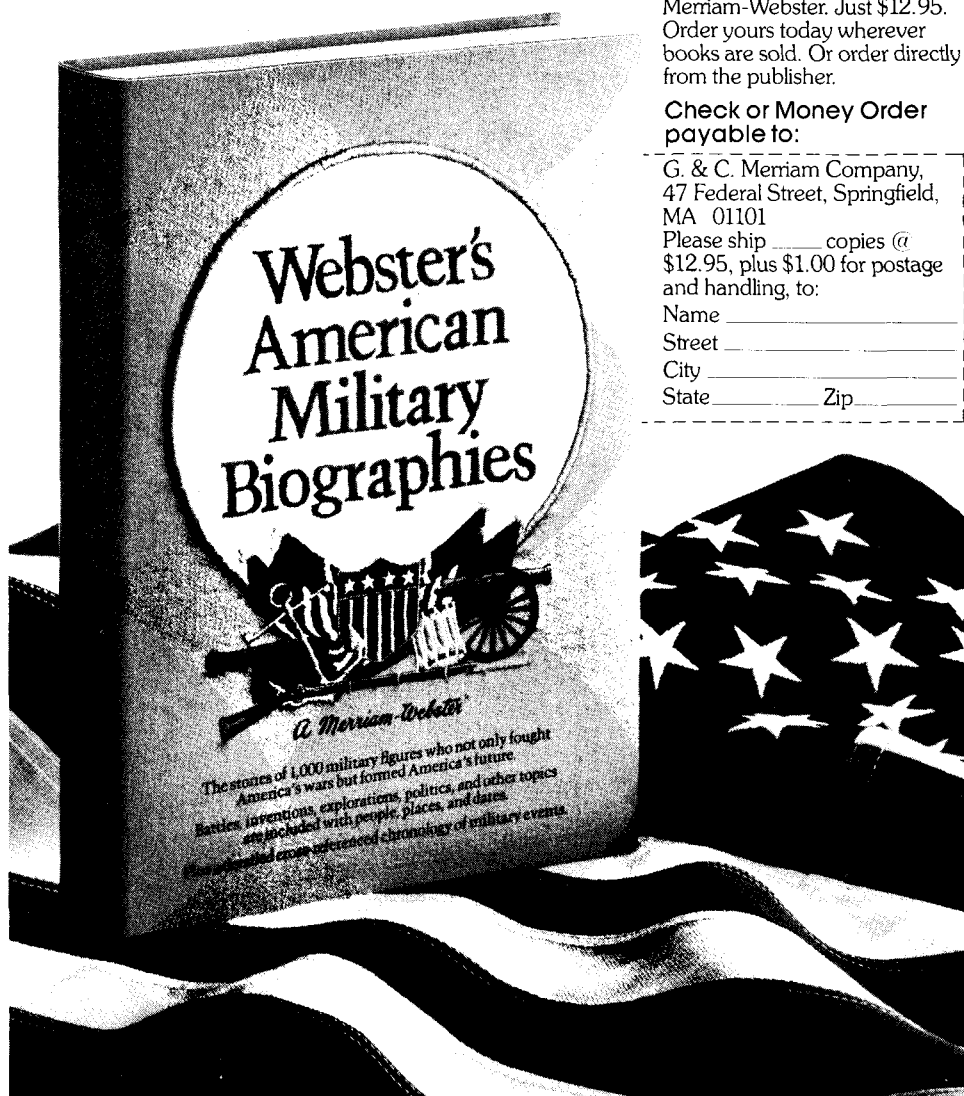
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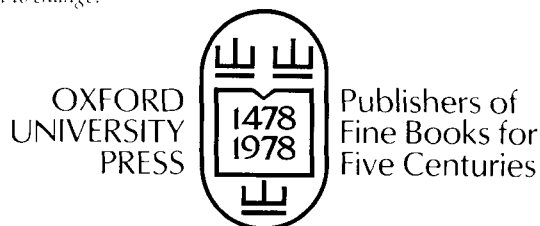
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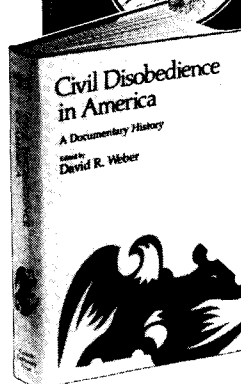
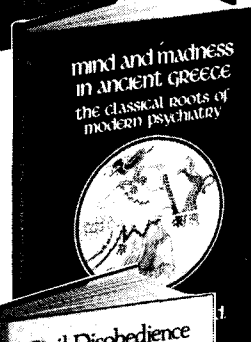
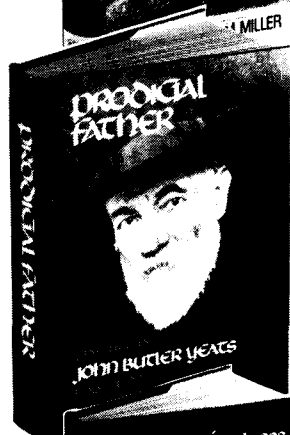
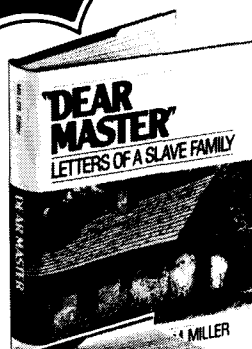
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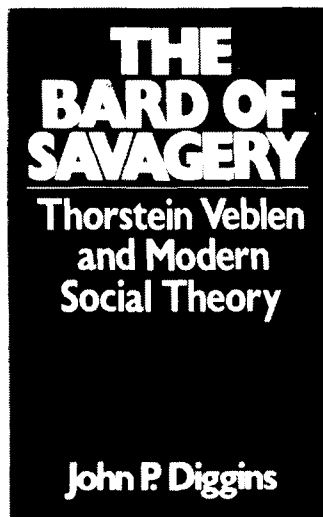
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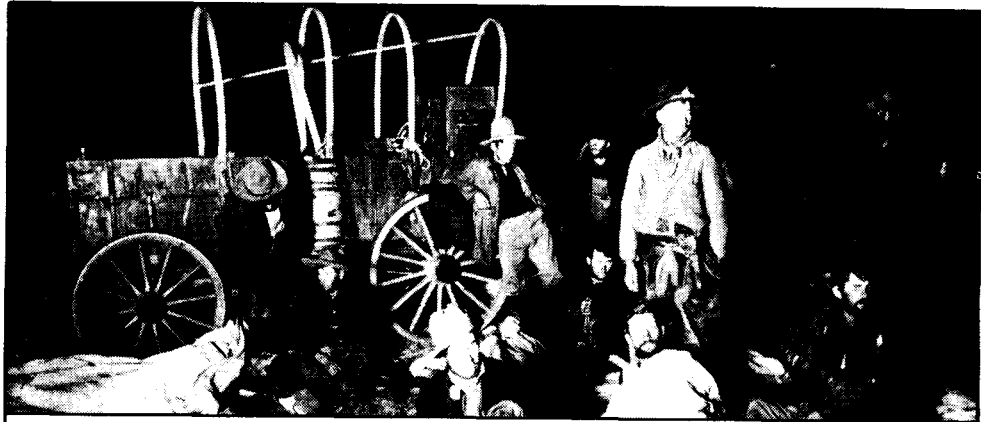
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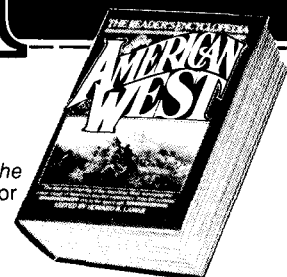
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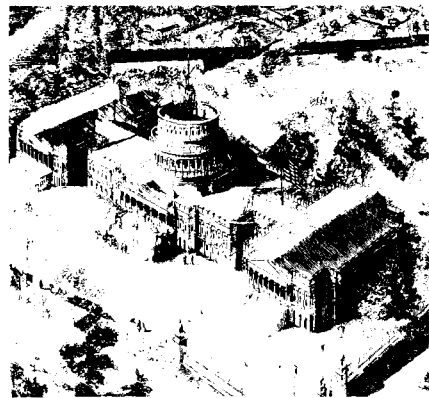
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